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Essays on Education, Inequality and Society

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For Christopher: amor vincit omnia.

“If you aren't in over your head, how do you know how tall you are?”

— T.S. Eliot

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Essays on Education, Inequality and Society

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This dissertation consists of three chapters on labor economics. The first two chapters focus on education, and the third examines inequality and incarceration. Chapter one explores whether college students strategically delay exiting college in response to poor labor market conditions. It exploits variation in U.S. state unemployment rates to identify the causal impact of unemployment rates on time to graduation. Strategic delay is observed among both men and women. Results indicate that students delay graduation by approximately 0.4 months for each percentage point increase in junior-year unemployment rates, implying the average student delays by approximately half a semester during a typical recession. Effects are greatest for men with freshman majors in education, professional and vocational technologies, the humanities, business, and the sciences, and for women in education, the sciences, or undeclared. Delays are robust to fluctuations in students' in-school work hours, earnings, and job market conditions.

Chapter two assesses the impact of over-the-counter access to emergency contraception on women's educational attainment using variation in access produced by

state legislation since 1998. Approximately 5% of American women of reproductive age experience an unintended pregnancy annually, indicating a significant unmet need for contraception. Results indicate that cohorts with greater access to emergency contraception are more likely to graduate from high school and attain the associate's degree. Effects for high school graduation are most pronounced among black women, while increases in associate's degree attainment are driven primarily by white and Hispanic women.

Chapter three explores the relationship between incarceration and generational inequality. Using a calibrated OLG model of criminal behavior with race, inheritance and endogenous education, I calculate how much longer prison sentences, and a higher likelihood of capture and conviction contribute to income inequality. Results indicate that changes to criminal policy mirroring those of the "tough on crime" legislation of the 1980s and 1990s, including an 18% increase in criminal apprehension and a 68% increase in prison sentence length, have little impact on inequality as measured by the Gini coefficient. Instead, the model provides evidence that these enhanced enforcement measures deter crime and decrease incarceration rates.

Table of Contents

List of Tables

List of Figures

Chapter 1

Waiting It Out? Unemployment and Strategic Delay Among College Students

Recent work indicates that student cohorts exiting college in times of high unemployment suffer persistent reductions in income, with slow catch-up to more fortunate cohorts. Significant negative earnings effects from entering the workforce during a poor job market have been found for college graduates (Kahn 2010; Oreopoulos, von Wachter and Heisz 2008, 2012), economics Ph.D. graduates (Oyer 2006), MBA graduates (Oyer 2008), and German workers at all education levels (Bachman, Bauer and David 2010).[1] Given the transience of typical business cycle fluctuations, a small change in graduation or dropout timing could significantly alter a worker's career and income path. In light of this, do college students strategically delay their college exits or change their training plans in response to poor labor market conditions? Optimal responses to a depressed job market may include extending college training to outlast a downturn, adding a major or switching to a more marketable field, or going to graduate school to gain a competitive edge. As higher chances of unemployment, underemployment and job mismatch lower the opportunity cost of remaining in school, waiting it out becomes more

[1] Oreopoulos et al (2008) summarize this literature.

attractive. However, little research to date has explored whether college students respond strategically to the job market they expect after leaving school.

This paper exploits variation in U.S. state unemployment rates over time to identify the effect of unemployment on time to college graduation using data from the 1995 and 2003 waves of the Beginning Postsecondary Students Longitudinal Study (BPS).[2] I find significant evidence of strategic delay behavior associated with unemployment rates observed while in college.[3] Students observing an increase in unemployment for young college-educated workers during their junior year delay graduation by between 0.48 and 0.88 months among men, and between 0.41 and 0.48 months among women, for each one percentage point increase in unemployment rates. In a recession characterized by a 4 percentage point increase in unemployment, this implies the average student delays graduation by nearly half a semester. Students in certain areas of study delay significantly more than average: for example, men majoring in education or professional and vocational technologies (such as communications and architecture) remain in college nearly 2 months longer for each percentage point increase in unemployment. These delays are robust to fluctuations in students' in-school work hours, earnings, and job market conditions, which supports the interpretation of unemployment-related delay as a strategic response. Job market conditions also magnify the effect of financial constraints, student aid and tuition on students' time-to-graduation.

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[2] Due to data limitations, I am unable to address whether students respond to poor job market conditions by switching majors, double-majoring or attending graduate school. Investigating these important outcomes is part of my ongoing research agenda.

[3] For simplicity throughout, the term "college" denotes a 4+ year, BA-granting institution of higher learning.

Time to graduation and training choice have important implications for college administrators, employers, and policy makers. Student's exit and training choices directly impact college resources available to serve incoming classes. Institutions dependent on endowment income or public financing, as well as those statutorily required to meet admissions quotas, may find themselves especially pinched if student exit delay coincides with decreasing revenues. Furthermore, pervasive delay or training change may shift workforce composition or skill set by changing the size, ability or training profile of graduating cohorts.

1.1 Related Literature

Empirical evidence demonstrates that entering the workforce during an economic downturn has a significant and persistent negative impact on workers' job quality and earnings. Much recent work has focused on new college graduates. For example, Oreopoulos, von Wachter and Heisz (2008, 2012) demonstrate that Canadian young men graduating during a recession experience reductions in income over an extended period, with a cumulative loss of earning at about 5% for a typical graduate. They find that high-quality graduates (e.g., those from more prestigious colleges or majors) recover more rapidly than others, as they quickly move to better paying firms. Wage recovery for the average recession graduate occurs over a decade, while lower-quality graduates may suffer permanently reduced incomes. Similarly, in her study of the 1982 recession's impact on NLSY79 respondents, Kahn (2010) finds that white men graduating under poor state and national employment conditions suffer a significant and permanent decline

in wages, and work in lower-level occupations. Using unemployment rates in the year of college graduation, she shows that recession graduates are more likely to obtain a graduate degree than those exiting college in better times.[4] Because she seeks to document the effects of unemployment experienced just after graduation, Kahn (2010) recognizes the potential endogeneity of graduation year and instruments, intentionally washing out any strategic exit timing by undergraduates. The difference between Kahn's (2010) OLS and IV results gives early evidence that students do intentionally time their exits. Documenting and analyzing this strategic manipulation is precisely the goal of this paper.

Despite the importance of a worker's early job market experience to his career path, relatively little work to date explores how college student behavior changes in response to economic conditions. To my knowledge, this is the first research to consider whether students strategically manipulate their graduation timing in accordance with observed unemployment.[5] In their study of youth exiting Norway's two-tiered secondary education system, Raaum and Røed (2006) find that adverse local job market conditions at graduation significantly increase youth unemployment and cause students to delay leaving school, but not to obtain more training (that is, low-tier secondary students do not switch to high-tier education tracks). The current paper poses a similar question for

[4]Although Kahn (2010) postulates that young workers facing poor conditions may be better off to delay job market entry, on average fewer than 50 observations per graduation year remain in the analysis set after identifying college state and applying other inclusion criteria. The data therefore lack the statistical power for further exploration.

[5]Several general models of workforce entry/reentry timing to macroeconomic conditions exist, and may shed light on college student's decision mechanisms. For example, Akerlof, Rose and Yellen (1990) model voluntary unemployment driven by wage persistence, which produces strategic waiting after lay-off during a recession. Workers choose short-term unemployment until better jobs become available during the recovery. Using data from the NLSY79, they provide empirical evidence of pro-cyclical wage persistence based on entry wage among those remaining in jobs relative to movers. In this framework, workforce entry delay while in college is analogous to voluntary unemployment.

college students. Others have considered how contemporaneous work and study impact graduation timing. For example, Messer and Wolter (2007) develop a model of time to degree wherein students optimize the length of study based on the cost and consumption benefit of time in college, and the value of future work. In accordance with their model, they provide evidence that time to graduation among Swiss college students decreases with rising unemployment and interest rates in the student's third and fourth years; they argue that these factors incentivize students to focus on their studies by reducing work distractions, and increase the cost of attendance. Although they do consider the impact of employment conditions on time to graduation, Messer and Wolter (2007) treat the student's job market interaction as facilitating his college consumption benefit, and not as a strategic workforce entry decision.

Students may stay shorter or longer in college for many reasons unrelated to perceived employment opportunities, including student ability, the need to work for income while enrolled, and financial aid type or generosity; evaluation of time-to-graduation therefore requires rich data.[6] Students enjoying tuition or living expense subsidies will have greater incentive to delay. On the other hand, if a five-year bachelor's degree carries a negative stigma, students may prefer to graduate on time. In a sample of Italian students, Aina and Pastore (2012) find that late college exiters suffer a penalty equal to 7% of the median wage, and hypothesize that delayed graduation signals low

[6] Cohort size may also matter to graduation timing, if students strategically respond to the increased competition present in large cohorts. Evidence provided by Wachter and Wascher (1984) suggests that workers on the cusp of a baby boom tend to accelerate their education to enter the job market before the wave peaks, while those on the young end of the boom delay job market entry by extending their education. However, it seems unlikely that these effects will occur without significant variation in cohort size.

student ability. Decreases in student preparedness and aptitude have also increased average time to degree, as documented by Bound, Lovenheim, and Turner (2007). I account for student ability using SAT scores[7] and high school GPA. Major choice also provides an indication, however noisy, of student ability.

Financial constraints also provide significant motivation to graduate. To the extent that recessions impact student finances through the student's work, family support, financial aid generosity, or loan interest rates, downturns may introduce simultaneity into the student's graduation timing decision, with potentially heterogeneous effects depending on student characteristics. If high unemployment leads to a reduction in earnings, then students supporting themselves may lack funds to enroll a given semester and thus extend their time to degree, while students from more affluent backgrounds may fall back on family support, choose not to work, and thus graduate faster.[8] Disentangling student's strategic graduation timing behavior from other sources of delay depends critically on controlling for factors impacting the student's budget constraint, and their interactions with job market conditions. Finally, recessions may also impact institutional resources in ways which inhibit student progress beyond financial aid. Bound, Lovenheim, and Turner (2007) find that erosion in student-to-faculty ratios has contributed to the observed increase in time to degree in recent years, particularly at lower-ranked public universities. I account for these factors using a rich dataset of student

[7] I normalize SAT scores by wave to compensate for the 1995 re-centering of the exam.

[8] Student employment will increasingly influence time to graduation as more students work. Scott-Clayton (2012) finds that recent college cohorts work more frequently and longer hours than past cohorts, and that college student labor supply responds to market conditions. Providing more generous aid to reduce student's need to work may prove a double-edged sword. Though differences between the American and European college systems dampen comparison, Brunello and Winter-Ebmer (2003) find that European students in more generous aid environments finish more slowly.

and university characteristics, and consider interactions between unemployment rates and student ability, financial aid, family income, outside work habits, course of study, and school size and type.

Which students strategically delay in college may also depend on the wage mechanism at work in the employment market they will enter. Prolonged effects from a less-than-robust job market entry can arise under a variety of theoretical constructs, with different implications for which students are most impacted. For example, if firms use past wages as a signal of worker ability as in Devereux (2002), then unemployment immediately following graduation may set a young worker on a low-level earnings path for an extended period of time. The same result could arise if early on-the-job human capital accumulation matters to the future earnings path (as summarized by Kahn 2010). If firms observe even a noisy measure of student ability when offering first positions, students under a certain quality threshold may have incentive to delay graduation in hopes of pooling with higher-ability students when demand for young workers rebounds. Although in theory ability-related delay may vary across the business cycle, I find no evidence that this occurs. Therefore, the value of time to degree as a signal of ability degrades in times of high unemployment for young educated workers.

1.2 Empirical Strategy

My empirical strategy for estimating the impact of unemployment on student behavior exploits variation in U.S. state unemployment rates for young, college-educated workers across two waves of the Beginning Postsecondary Studies Longitudinal Survey

(BPS), which surveyed students beginning college for the first time in 1995 and 2003.[9,10] One may expect a sophomore observing high unemployment to respond differently than a senior. As the impact of job market conditions on student behavior likely depends on student progress in college, I estimate the effect of unemployment on degree completion relative to academic-year unemployment rates, for each of the first four years of a student's college experience.[11]

To assess how student time-to-graduation varies with job market conditions, I estimate months to graduation for the subsample of students graduating within 6 years using the OLS[12] specification

$$M_{ist} = \beta_1 + \sum_{j=1}^{4} \beta_2^j UR_{s,t+j-1}^{PC} + \sum_{j=1}^{4} \beta_3^j UR_{s,t+j-1}^{IC} + X_i\gamma + W_t\varpi + \eta_s + \rho_t + \varepsilon \qquad (1.1)$$

where M_{ist} represents time to graduation in months for student *i*, who began attending college in state *s* in year *t*. To avoid conflating the effects of unemployment with state or time trends in time to graduation, I include fixed effects η_s for the student's college state *s,* and ρ_t for the student's year of entry into higher education *t*. I cannot include survey

[9] I do not use the 1989 wave of the BPS, as follow-up is available at 5 years from college start instead of 6 years, and a significant number of observations are missing key variables in a manner correlated with college selectivity.

[10] Wozniak (2010) finds that state labor demand shocks have a larger impact on location choice and a smaller effect on earnings for those with at least a college degree than for other workers. If students refer to the national job market instead of the market in their college state, then my use of state rather than national unemployment rates will tend to attenuate any findings, as students with a BA would tend to put less weight on the state job market in their optimization decision. Furthermore, as my model includes state fixed effects, I am estimating the impact of a state's deviation from the national average unemployment rate.

[11] Annual unemployment rates are calculated based on an academic year ending in May. Although a leading indicator of employment conditions (such as vacancies) may be preferable to the observed unemployment rate for this application, BLS data on job openings and labor turnover (JOLTS) are only available on the regional level. Using such data would result in a significant loss of variation for identification.

[12] Duration modeling is another natural strategy for this question. I present OLS results as they are immediately interpretable by a broad range of readers. Results for a discrete-time duration model are presented in Appendix B for the populations of all students, and all students not dropping out by 6 years. The results are generally consistent with the OLS results in the main paper.

year-by-college state fixed effects as such terms would align with the variation in unemployment rates. For college state *s*, unemployment rates among individuals aged 20 to 34 with a college degree $UR^{PC}_{s,t+i-1}$ (the "post-college" market), and for those with a high school diploma but no college degree $UR^{IC}_{s,t+i-1}$ (the "in-college" market), are calculated for the 12 months ending in May of each year, $i-1$ years after the student's entry into college in year *t*.[13] Students who work while in college likely participate in the employment market for young, high school-educated workers without a college degree. Unemployment rates for the in-college market (UR^{IC}) are included in the model to absorb any delay attributable to student work activity while in school. This allows captures of the student's strategic response to fluctuations in the job market they can expect to enter upon graduation (UR^{PC}). As approximately 70% of college graduates remain in their college state at five years after graduation (Kodrzycki 2001), college state unemployment rates generally represent conditions faced by graduates in their early careers. While Kodrzycki (2001) also finds that movers choose states with more robust job markets, her regression results indicate that graduate's personal characteristics matter more in determining who migrates than do overall college state employment opportunities.

Matrix X_i in Equation (1.1) contains student-level variables fixed at the time of student entry into college, such as entrance exam scores, starting major, financial status, and demographics. W_t contains indicators for the survey strata of each survey wave, to

[13] For example, the freshman year unemployment rate matched with a student beginning college in September of 2003 is based on college state unemployment data from June 2003 to May 2004. Earlier versions of this paper used unemployment rates in four-month increments with largely the same results. However, as small CPS sample sizes in many of the less populated states resulted in less reliable unemployment data for those areas, I use annual rates here.

avoid possible bias resulting from the sampling scheme used to select students for survey participation.[14] All variables based on the student's college institution, including college state and unemployment rates, reflect the student's freshman college location, regardless of possible out-of-state transfer later on. Although the data include month and year of college entry and exit, actual months in class (excluding breaks) is unavailable for the 1995 wave. Because approximately 20% of the colleges in the study sample do not follow the semester system, grouping student enrollment spells into periods longer than a month introduces unnecessary noise.

One hurdle to isolating the effect of job market conditions on college student's strategic exit timing decisions using the post-college job market (UR^{IC}) is the possibility that college-educated unemployment shocks may impact the student's budget constraint, through the parent's employment. If the student's parents participate in a job market which is correlated with the student's post-college market UR^{IC}, an employment shock in the parent's market may spread to the student's budget constraint and interfere with the simple interpretation of coefficients β_2^i in Equation (1.1) as the student's strategic response to her expected job market. A similar transmission mechanism could also affect the observed response to the in-college job market, captured by coefficients β_3^i in

[14] As with any survey data, careful consideration of the sampling structure is important to avoid endogenous sampling bias in the results. The BPS sampling strata (over which the probability of selection varies) differ notably between survey waves. As participant selection predates all outcomes I consider, there is no risk of direct endogenous sampling. However, many sample strata align over characteristics potentially correlated with outcomes of interest. For example, samples in all years were drawn differently for public, private non-profit, and private for-profit institutions. It is not unreasonable to suspect that time to graduation will vary over institutional control. To account for potentially outcome-aligned sampling strata, I include dummies for all strata in W_t, separately for each wave. In the second wave only, the data include the inverse probability of selection, enabling a test of the strata dummy method for lingering endogenous sampling bias and misspecification. I find no evidence of endogenous sampling or misspecification when including the full set of strata dummies W_t. See Solon, Haider and Wooldridge (2013) for a recent discussion of weighting.

Equation (1.1). If family budget pressures cause graduates to reduce their lifetime tuition outlays by graduating faster, the signs of coefficients β^i may become negative, or alternate sign between exposure years depending on whether the budget effect of unemployment outweighs its strategic delay impact in a given year. Whether this parent-shock contamination is likely to present in a given year depends on the extent of correlation between the job markets for older and younger workers, how important family resources are to the student's budget, and student's information on his future job market. This empirical matter is investigated further when discussing the results in Section IV.

To complement the main analysis, I introduce interactions of the post-college unemployment rate and key covariates, including normalized SAT score, high school GPA, freshman major area of study, tuition, student work hours during the semester, student income and aid, and whether the college is private or public. The interaction analysis identifies effects for sub-populations of interest to colleges and policy makers, and also functions as a robustness check to the claim that student delay response to the post-college job market is in fact strategic. One may suspect that economic shocks more frequently disrupt the studies of working students or those with low levels of family income. Alternatively, perhaps students from wealthier backgrounds will delay more frequently, as they have greater resources to support the college lifestyle and may face less pressure to begin supporting themselves. It may also be that only students of a certain ability profile will benefit from delay. For example, if the highest ability graduates can obtain good jobs despite the poor market, then the ability threshold at which graduation pooling occurs may change with job market conditions. Analysis of student work

behavior and other financial variables will prove especially important to eliminating alternative channels for the effect of unemployment rates on time to graduation.

A potential pitfall in assessing time to graduation arises if job market conditions produce selection on which students persist in college. Depending on the channels through which unemployment influences a student's choice set, job market conditions during school may impact whether and when college students drop out. As the BPS data are nationally representative of first-time students at 4-year colleges, the classic problem of selection by sample omission does not arise; however, if the desired reference group is freshman entering four-year colleges, then we must evaluate whether college persistence is correlated with unemployment conditions (as those who do not persist cannot graduate). It is unclear *a priori* whether high unemployment will influence whether students drop out.[15] To determine whether a persistence-selection problem exists, I regress an indicator for dropout by the 6th year after entering college on the covariates described in Equation (1.1) using a linear probability model. I find that fluctuations in the in-college unemployment market (the plausible outside option for dropouts) have no impact on men or women's likelihood of dropout by 6 years from college start, and that an increase in post-college unemployment in the freshman and sophomore years decrease men's chance of drop-out by 2.7% and 1.7% (respectively) per percentage point increase in unemployment rates. Overall, it is unlikely that this small dropout response has a

[15] I also evaluate whether unemployment conditions impact how long eventual dropouts remain in college before exiting. I find that an increase of one percentage point in freshman-year unemployment rates in the in-college market causes men to drop out 2.6 months (se 1.267) earlier *ceteris paribus*, but has no significant impact on how long women dropouts persist before exiting.

significant selection impact on the delay of graduation results for men, as it is unclear that these incremental persisters will graduate within the 6-year period observed.[16]

Finally, a large literature documents significant differences between men and women's job market participation, occupational distributions, and returns to education.[17] These distinctions reach back into the college years. For example, among Canadian college students, Andres and Adamuti-Trache (2007) find significant and persistent field of study and completion rate differences between males and females enrolling between 1979 and 2004. As a result, all analyses are preformed separately for men and women.[18]

1.3 Data

The individual-level data come from waves 2 and 3 of the Beginning Postsecondary Students Longitudinal Study (BPS), which interviewed 17,071 first-time college students beginning as freshmen at 4-year schools in 1995 and 2003 at one and six years from entrance. [19] These restricted-access data include information on student demographics, entrance exams, financial aid, persistence, areas of study, work experience during and after college, degree attainment, and personal finances. Institutional characteristics including college state, control type (public, private non-profit, or private

[16]For completeness, duration analysis of time to graduation is included in Appendix B. Because it takes as it analysis population the full study sample of entering freshmen, regardless of college exit outcome, it is entirely immune from unemployment-related selection via dropout. However, such a model cannot quantify delay as simply as OLS.

[17]For example, see Goldin (1999), Blundell and MaCurdy (2000), Bayard, Hellerstein, Neumark and Troske (2003), and Andres and Adamuti-Trache (2007).

[18]For the BPS data used herein, I find that male and female students differ to such a degree that when including both in a single regression framework, the competing effects either swamp each other and mask the result, or the strength of one relationship falsely attributes a result to both sexes when in actuality it only holds for one.

[19]The surveys also included 8,876 students beginning at 2-year colleges, and 2,720 attending less than 2-year schools.

for-profit), educational level, and highest degree offered are also available. The data result from a combination of student interviews, institutional records, the student's FAFSA filings, and official exam records. I focus on US citizens between the ages of 17 and 19 when starting school, beginning their studies at public or not-for-profit four-year institutions located in the United States, and who entered college in the fall semester.[20] Student records must also include the college state, institution type, graduation status, SAT or ACT score, race, initial major, college start date, and degree date (if the student graduated). I exclude 275 students who graduated in 3 years or less, as these individuals likely have unobserved differences in ability or preparation. Of the 12,271 individuals in the study sample, 8,142 have graduated, 2,276 are still enrolled, and 1,853 have dropped out (exited without return or degree) by six years after starting college. Dates of college start and exit are available at the month level. While the semester-based calendar is most common among the surveyed institutions, approximately 20% of the study sample individuals are at institutions on other calendars.

Table 1.1 describes the study sample, and displays important differences between men and women graduates and non-graduates. As the analysis accounts for the survey structure by including indicator variables for the sample cells, the means presented in Table 1.1 are unweighted.[21] On average, students graduating within 6 years are less likely to self-identify as a minority, have higher SAT scores, are more likely to attend college

[20]Individuals beginning their studies in the summer or spring semesters differ in racial composition, family financial background, Pell grant status, and SAT score from fall starters. I exclude them to avoid confounding the results with unobservable differences between early, late and fall starters. After accounting for other exclusions, late and early starters total 1,313 individuals, or approximately 10% of the sample prior to their exclusion.

[21]There is no statistically significant difference between weighted and unweighted versions of Table 1.1. See Footnote 14 for additional discussion of the BPS survey structure.

out of their home state, and are less likely to attend a public institution than non-graduates. Graduators enjoy higher family resources as measured by freshman Expected Family Contribution (EFC), lower rates of freshman Pell grant receipt, pay higher tuition, and work less than those not graduating in 6 years or less. Generally, graduating students have equal or lower levels of federal student aid but higher amounts of total student aid than those not graduating, indicating higher grant and scholarship receipt among graduates. Those graduating are also less likely to have changed majors than those not graduating. Overall among those graduating, 58% complete at 4 years, 31% at 5 years and 11% at 6 years from freshman enrollment. 45% of those not graduating have left college without return by 6 years from entrance. Of students exiting without return or degree within 6 years, 66% exit in 4 years or less, 22% exit in year 5, and 12% exit in year 6.

Among those graduating in 6 years or less, men and women differ in meaningful ways. Female graduates' freshman EFC is approximately $2,000 lower than that of male graduates. Females are also more likely to receive a Pell grant. These facts generally indicate that the typical female graduate comes from a household with lower income than the typical male graduate; however, men and women receive essentially equivalent freshman financial aid. Women are more likely to work as freshman and work more hours per week, but have slightly lower annual earnings than men. The expected gender differences in major choice are present, with women less likely than men to begin in computing, engineering and math or business majors, and more likely to begin in humanities, health or education.

Table 1.1: Descriptive Statistics by Gender and 6-Year Graduation Status

	Men						Women					
	Graduating		Not graduating		Total		Graduating		Not graduating		Total	
Variable	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*
N	3,332		2,012		5,344		4,810		2,117		6,927	
Months to BA	51.10	8.04	--	--	51.10	8.04	49.60	7.48	--	--	49.60	7.48
Graduation within 4 years	0.52	0.50	--	--	0.33	*0.47*	0.62	*0.48*	--	--	0.43	*0.50*
Graduation within 5 years	0.87	0.33	--	--	0.54	*0.50*	0.90	*0.29*	--	--	0.63	*0.48*
Graduation within 6 years	1.00	0.00	--	--	0.62	*0.48*	1.00	*0.00*	--	--	0.69	*0.46*
Dropout within 4 years	--	--	0.29	*0.45*	0.11	*0.31*	--	--	0.30	*0.46*	0.09	*0.29*
Dropout within 5 years	--	--	0.40	*0.49*	0.15	*0.36*	--	--	0.39	*0.49*	0.12	*0.32*
Dropout within 6 years	--	--	0.46	*0.50*	0.17	*0.38*	--	--	0.44	*0.50*	0.13	*0.34*
White	0.82	*0.38*	0.74	*0.44*	0.79	*0.41*	0.81	*0.39*	0.72	*0.45*	0.78	*0.42*
Black	0.05	*0.22*	0.11	*0.31*	0.07	*0.26*	0.07	*0.26*	0.14	*0.35*	0.09	*0.29*
Hispanic	0.05	*0.21*	0.08	*0.27*	0.06	*0.23*	0.05	*0.21*	0.08	*0.27*	0.06	*0.23*
Other race/ethnicity	0.08	*0.28*	0.07	*0.26*	0.08	*0.27*	0.07	*0.26*	0.07	*0.25*	0.07	*0.26*
Age as freshmen	18.3	*0.48*	18.3	*0.48*	18.3	*0.48*	18.2	*0.46*	18.3	*0.47*	18.2	*0.46*
SAT/ACT score	1,103	*197*	978	*202*	1,056	*208*	1,059	*192*	923	*188*	1,018	*201*
High school GPA	3.41	*0.44*	3.08	*0.57*	3.29	*0.52*	3.48	*0.40*	3.19	*0.52*	3.39	*0.46*
College not in home state	0.31	*0.46*	0.21	*0.41*	0.27	*0.44*	0.29	*0.45*	0.20	*0.40*	0.26	*0.44*
Public college	0.53	*0.50*	0.69	*0.46*	0.59	*0.49*	0.53	*0.50*	0.67	*0.47*	0.57	*0.49*
Religious college†	0.25	*0.43*	0.17	*0.38*	0.21	*0.41*	0.24	*0.43*	0.18	*0.39*	0.22	*0.41*
College enrollment	14,816	*12,542*	14,568	*12,668*	14,723	*12,589*	14,115	*12,456*	13,770	*11,941*	14,009	*12,301*

Table 1.1 continues on the following page.

Table 1.1, Continued: Descriptive Statistics by Gender and 6-Year Graduation Status

	Men						Women					
	Graduating		Not graduating		Total		Graduating		Not graduating		Total	
Variable	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*
Freshman year work and finances; monetary variables in thousands												
Expected Family Cont.	16.4	*17.8*	11.2	*13.7*	14.4	*16.6*	14.7	*17.1*	9.6	*13.3*	13.1	*16.2*
Household AGI	78.7	*53.0*	61.7	*45.0*	72.4	*50.9*	74.8	*52.1*	56.9	*44.2*	69.4	*50.5*
Federal student aid	3.9	*5.3*	3.9	*4.7*	3.9	*5.1*	3.8	*5.1*	4.1	*4.7*	3.9	*5.0*
Total aid	8.7	*8.7*	6.5	*6.7*	7.9	*8.1*	8.8	*8.5*	7.1	*7.1*	8.3	*8.1*
Pell receipt	0.17	*0.37*	0.26	*0.44*	0.20	*0.40*	0.20	*0.40*	0.33	*0.47*	0.24	*0.43*
Pell amount (recipients)	2.13	*1.30*	1.94	*1.22*	2.03	*1.27*	2.02	*1.26*	2.02	*1.28*	2.02	*1.27*
Tuition	12.37	*8.94*	7.68	*6.68*	10.63	*8.48*	11.85	*8.82*	7.51	*6.62*	10.55	*8.46*
Job hours worked/week	8.6	*11.3*	13.3	*13.5*	10.4	*12.4*	9.2	*10.9*	14.6	*13.5*	10.8	*12.0*
Has job?	0.54	*0.50*	0.65	*0.48*	0.58	*0.49*	0.60	*0.49*	0.71	*0.45*	0.63	*0.48*
Job earnings	2.02	*4.50*	2.87	*4.12*	2.36	*4.37*	1.99	*3.73*	2.95	*4.07*	2.30	*3.87*
Freshman major category												
Undeclared	0.31	*0.46*	0.29	*0.45*	0.30	*0.46*	0.31	*0.46*	0.29	*0.46*	0.31	*0.46*
Humanities	0.07	*0.26*	0.10	*0.30*	0.08	*0.27*	0.10	*0.30*	0.10	*0.31*	0.10	*0.30*
Social sciences	0.07	*0.25*	0.05	*0.22*	0.06	*0.24*	0.09	*0.28*	0.08	*0.27*	0.08	*0.28*
Life/physical sciences	0.09	*0.29*	0.07	*0.25*	0.08	*0.28*	0.08	*0.28*	0.07	*0.26*	0.08	*0.27*
Engineering/computing/math	0.17	*0.38*	0.17	*0.38*	0.17	*0.38*	0.04	*0.19*	0.03	*0.18*	0.04	*0.18*
Education	0.03	*0.18*	0.05	*0.22*	0.04	*0.19*	0.11	*0.31*	0.10	*0.31*	0.11	*0.31*
Business	0.14	*0.34*	0.13	*0.33*	0.13	*0.34*	0.09	*0.29*	0.10	*0.30*	0.10	*0.29*
Health	0.03	*0.18*	0.04	*0.20*	0.04	*0.19*	0.10	*0.30*	0.14	*0.34*	0.11	*0.32*
Professional/vocational tech.	0.08	*0.27*	0.10	*0.30*	0.09	*0.28*	0.07	*0.26*	0.07	*0.26*	0.07	*0.26*

Notes: Data unweighted. Financial variables in thousands. Data are drawn from the Beginning Postsecondary Students Longitudinal Study of first-time college students beginning as freshmen in 1995 or 2003, and limited to those entering at age 17-19 and graduating by June of their 6th year after entry. Sample restricted to US citizens beginning their studies at 4-year colleges located in the United States and DC, entering college in the fall, with non-missing start date, degree date, home state, institution type, graduation status, SAT/ACT score, race, and initial major. The professional and vocation technologies category includes majors such as law enforcement/security, communications, architecture, and construction management.
†College's religious affiliation available for 1995 wave only.

Table 1.2: Unemployment Rates by Age and Education

Unemployment	Percentiles 10th	50th	90th	Mean	Std. Dev.
Panel A: Student's Job Markets					
Workers aged 20-34 with BA or higher					
Freshman year	1.35	2.66	4.47	2.82	*1.23*
Sophomore year	1.30	2.49	3.89	2.65	*1.15*
Junior year	0.89	2.26	3.67	2.33	*1.07*
Senior year	0.95	2.05	3.18	2.13	*1.04*
Workers aged 20-34 with a high school diploma, but no BA					
Freshman year	4.24	6.51	9.11	6.56	*1.99*
Sophomore year	4.18	6.09	8.10	6.20	*1.87*
Junior year	3.81	5.52	8.05	5.78	*1.79*
Senior year	3.30	5.11	7.46	5.33	*1.79*
Panel B: Parent's Job Markets					
Workers aged 35-64, with at least a high school diploma					
Freshman year	2.15	3.22	4.75	3.39	*1.00*
Sophomore year	2.28	3.32	4.52	3.38	*1.01*
Junior year	1.98	2.96	4.20	3.05	*1.02*
Senior year	1.66	2.52	4.06	2.65	*0.92*

Notes: Weighted data from CPS outgoing rotation groups from 1994 to 2012.

To precisely describe the job markets facing students, I use CPS outgoing rotation groups from 1994 to 2012 to calculate annual state unemployment rates for workers aged 20 to 34 in two education groups: those with a high school diploma (or equivalent) but no BA, and those with a BA or higher. Percentiles, means, and standard deviations for these youth unemployment rates are presented in Panel A of Table 1.2. For reference, Panel B presents unemployment rates for workers aged 35-64 with at least a high school diploma. While unemployment throughout the data period is low, rates nonetheless exhibit

adequate cross-state variation for identification.[22] As shown in Table 1.2 Panel A, rates for young high school graduates without a college degree display a higher level of volatility (in terms of standard deviation) than those for college graduates.[23] Youth unemployment in the post-college and in-college markets is moderately positively correlated: within-year correlation coefficients in student's freshman, sophomore, junior and senior years are 0.26, 0.31, 0.20 and 0.20 respectively (all significant at or above the 5% level).

Finally, I validate my method for unemployment rate computation by comparing calculated annual state-level unemployment rates for all workers to the state-level rates published by the Bureau of Labor and Statistics.[24] Differences between my annual rates and the official BPS rates are statistically insignificant.

1.4 The Effect of Unemployment Conditions Observed While in College

1.4.1 Time to graduation

Table 1.3 presents estimates of the impact of unemployment observed while in college on the dependent variable of months to college graduation for the population of

[22] As shown in Table 1.2, unemployment is generally low over both survey waves. While ideal data to address this question would include periods of recession and high unemployment, the richness of the BPS data used here allows accounting for student finances and ability to a degree no other data do, to my knowledge.

[23] The volatility in the high school educated unemployment rate is not an artifact of sample size. Its calculation is based on an average of 836 observations per state-by-year cell, while 347 observations per state-by-year cell are available for the college-educated unemployment rate.

[24] I test annual-level data to avoid timing discrepancies related to the CPS interview schedule, as well as noise related to political adjustments in the official BLS rates.

students graduating within 6 years. I include rates for both the "post-college" market, or market for young college-educated workers, and the "in-college" market, or market for young individuals with a high school diploma but no degree (coefficients β_2^i and β_3^i in Equation [1.1]). Because the relevant response timing structure is unknown, I first consider several different year-group specifications, using unemployment rates in college years 1-4.[25] Covariates include normalized high school GPA, normalized SAT score,[26] freshman year tuition, job hours worked while enrolled during freshman year, freshman expected family contribution and expected family contribution squared, as well as indicators for the student's race/ethnicity in 4 categories, freshman major in 9 categories, whether either of the student's parents obtained a Bachelor's degree, urbanity of the college location in 3 categories, college state, and survey sampling strata.[27] These comprise the standard covariates for all analyses herein. Early analysis also included student loans, total federal aid, Pell grant receipt status and amount, and earnings from work; these variables were excluded from the final model as they do not have a statistically significant impact on time to graduation (either individually or in unemployment rate interactions) when included with the standard covariates.

As shown in Table 1.3, men and women differ in their response to both the in-college and the post-college job markets. Time to graduation for both sexes is unaffected by unemployment rates experienced during the freshman year. In the sophomore year, the

[25]Results for unemployment in years 5 and 6 are not reliable as a large number of graduates exit college beginning in year 4.

[26]If only ACT score is available for a student, I translate the ACT score to an equivalent SAT score using the College Board's conversion matrix.

[27]Note that in the two survey waves used, the public/private status of an institution was an element of the sampling strata framework.

Table 1.3: Estimated Effect of Unemployment Rates on Months to College Graduation, College Years 1-4

(American college students beginning as freshmen in 1995 or 2003)

	Men		Women	
Mean state unemp. during student's	Post-College Market	In-College Market	Post-College Market	In-College Market
Freshman year	0.129	0.203	0.000	-0.046
	(0.245)	(0.250)	(0.210)	(0.242)
Sophomore year	-0.686***	0.214	-0.123	0.299
	(0.236)	(0.195)	(0.205)	(0.187)
Junior year	0.879***	0.093	0.479**	0.133
	(0.325)	(0.185)	(0.226)	(0.152)
Senior year	-0.403	-0.163	-0.169	-0.389***
	(0.254)	(0.127)	(0.240)	(0.136)

Notes: OLS coefficients with in standard errors parentheses. The dependent variable is elapsed months from college entrance to graduation. Men and women are regressed separately. Unemployment rates for all years in college and both in-college and post-college employment markets are included together in each regression. Regressions include controls for normalized high school GPA, normalized SAT score (or translated ACT score), freshman year tuition, job hours worked while enrolled during freshman year, freshman expected family contribution and expected family contribution squared, as well as indicators for race in 4 categories, freshman major in 9 categories, whether either of the student's parents obtained a Bachelor's degree, urbanity in 3 categories, college state, and survey sampling strata. Data are drawn from the Beginning Postsecondary Students Longitudinal Study of first-time college students beginning as freshmen in 1995 or 2003, and limited to those entering at age 17-19 and graduating by June of their 6th year after entry. Sample restricted to US citizens beginning their studies at 4-year colleges located in the United States and DC, entering college in the fall, with non-missing start date, degree date, home state, institution type, graduation status, SAT/ACT score, race, and initial major.
Significance indicated by --*: P<0.10, --**: P<0.05, --***: P<0.01 .

Table 1.4: Within-Year Correlation Coefficients for Unemployment Rates For Young and Old Workers

	Workers aged 35-64, with at least a high school diploma			
Youth unemployment rates	Freshman year	Sophomore year	Junior year	Senior year
Workers aged 20-34 with BA or higher	0.3911*	0.3910*	0.3852*	0.4737*
Workers aged 20-34 with a high school diploma, but no BA	0.6513*	0.7179*	0.6615*	0.7109*

Notes: Weighted data from CPS outgoing rotation groups from 1994 to 2012.
*Indicates correlation coefficient is significant at the 5% level or higher.

coefficient on the post-college unemployment rate for men is negative and significant, indicating that men who observe higher unemployment in the post-college market during their sophomore year actually graduate faster. Women are not likewise affected. In the junior year, both sexes respond to higher unemployment in the post-college market by extending their time to graduation, by 0.88 months for men and by 0.48 months for women per percentage point increase in unemployment. For men, the combined effect of unemployment experienced in the sophomore and junior years is 0.19 months delay; a test of joint significance strongly rejects the null hypothesis that this total equals zero (Prob > F = 0.0027). Finally, men do not significantly alter their time to graduation based on fluctuations in the in-college job market in any year, while women respond to higher unemployment in the in-college market during their senior year by graduating faster.

As discussed in the Empirical Strategy section above, transmission of shocks from the market for older workers to the student via his family's budget can produce negative estimates for coefficients on the youth unemployment rates in Table 1.3, and thus complicate interpretation of the coefficients. The sign of each coefficient thus depends on whether the effect of a parental shock or the strategic delay mechanism dominates in a given year. This is informed by how correlated youth unemployment rates UR^{IC} and UR^{PC} are to those for older adults,[28] the student's level of exposure to his future job market, and how important the family's budget is to the student's optimization problem. Controls included in matrix X_i for family income and student financial aid (both in the

[28] I do not segregate the job market for older adults by education because entry into high-paying occupations was far less dependent on college completion for previous generations than it is for today's youth.

freshman year) control for student's level of exposure to the parent's budget constraint to the extent possible given the data and the analysis environment. I discuss the other two factors below.

The relationship between youth and adult unemployment rates is described by Table 1.4, which presents within-year correlation coefficients for both youth unemployment rates and that for older workers. The observed correlations are universally larger than those between the two youth unemployment rates (discussed in the Data section above). Correlations between unemployment in the youth in-college market and that for older adults is particularly strong, and may contribute to the significant negative coefficient on the senior year in-college market for women (the coefficient for men is also negative in that year, although insignificant). Overall, a link between the youth and older adult employment markets appears substantiated, and a likely contributor to the negative coefficients observed in Table 1.3.

For many students, junior-year internships represent their first personal exposure to the employment market they can expect after graduation. What internship opportunities are available, how much competition students face in securing an internship, and how aggressively firms recruit on college campuses all provide students with direct information on conditions in their post-college target job markets. Furthermore, observing poor outcomes for older classmates as a younger student may not alter a rational student's behavior; as the typical recession lasts only 18 months, signals

Table 1.5: Estimated Effect of Junior and Senior Year Unemployment Rates on Months to College Graduation

(American college students beginning as freshmen in 1995 or 2003)				
Mean state unemp. during student's	Men		Women	
	Post-college market	In-college market	Post-college market	In-college market
Junior year	0.481*	0.002	0.411**	0.153
	(0.264)	(0.209)	(0.178)	(0.151)
Senior year	-0.190	-0.028	-0.038	-0.321***
	(0.215)	(0.140)	(0.233)	(0.115)

Notes: OLS coefficients with in standard errors parentheses. The dependent variable is elapsed months from college entrance to graduation. Men and women are regressed separately. Unemployment rates for the 3rd and 4th year in college, for both in-college and post-college employment markets are included in each regression. Regressions also include controls for normalized high school GPA, normalized SAT score (or translated ACT score), freshman year tuition, job hours worked while enrolled during freshman year, freshman expected family contribution and expected family contribution squared, as well as indicators for race in 4 categories, freshman major in 9 categories, whether either of the student's parents obtained a Bachelor's degree, urbanity in 3 categories, college state, and survey sampling strata. Data are drawn from the Beginning Postsecondary Students Longitudinal Study of first-time college students beginning as freshmen in 1995 or 2003, and limited to those entering at age 17-19 and graduating by June of their 6th year after entry. Sample restricted to US citizens beginning their studies at 4-year colleges located in the United States and DC, entering college in the fall, with non-missing start date, degree date, home state, institution type, graduation status, SAT/ACT score, race, and initial major.

of job market weakness in the freshman year are not as informative as later signals.[29] Depending on student's attention and information structure, it may be unreasonable to expect to observe strategic delay prior to the junior year.

In light of this, Table 1.5 presents results for the regression of months to graduation on unemployment rates in the junior and senior years only, together with the standard controls. As shown, the coefficient on the post-college junior year unemployment rate for men decreases to 0.48 months, commensurate with the coefficient for women of 0.41 months delay for an increase of one percentage point in post-college

[29]In a protracted downturn such as the Great Recession, whether this holds will depend on student's expectations of how long the recession will last at a given point in her college path.

unemployment. For men, this result is expected and reasonable, given the positive correlation between sophomore and junior year in-college unemployment, the large and negative coefficient observed in Table 1.3 for men's sophomore year, and the fact that the combined effect of the precise estimates in Table 1.3 was positive and significant. The coefficient on the senior year in-college market for women is essentially unchanged. Given the complexities of the optimization decision facing students and the various factors impacting their budget constraints, these estimates appear most plausible as approximations of student's strategic delay behavior with respect to the job market they can expect to enter after graduating.

1.4.2 Heterogeneity in the Effect of Unemployment Conditions Observed While in College

Interactions of the college-educated unemployment rates with selected covariates help illuminate the relationship between junior-year unemployment in the post-college market and student time to graduation. Appendix Tables A1 – A4 present results for interactions with the student's freshman major, high family income, financial aid status, and school size; other covariate interactions either confirmed the relationships discussed below, or did not produce statistically significant interaction terms for either sex.

Freshman major – Appendix Table A1 reports results from interacting junior year post-college unemployment rates with freshman major indicators. While individual interaction coefficients and main effects are statistically insignificant due to a lack of

power, marginal effects for certain majors can be precisely estimated. For men, an increase of one percentage point in junior-year post-college unemployment causes graduation delays of 1.3 months for those beginning in the humanities, 1.0 months for those in life and physical sciences, 2.0 months for those in education, 1.1 months for those studying business, and 2.0 months for those in professional and vocational technologies.[30] Among women, those beginning as undeclared delay 0.6 months, those in the life and physical sciences delay 1.2 months, and those in education delay 0.8 months for each percentage point increase in the junior-year post-college unemployment rate. As the typical recession results in a 4 percentage point increase in unemployment, delay for these majors quickly multiplies from months to entire semesters.

High family income – Appendix Table A2 reports results from interacting college-educated unemployment rates with an indicator for freshman family AGI in the 4th and 5th quintiles, which include those with income of $75k and higher. Interaction coefficients for both genders are negative and significant. Students from better-off families may be incentivized to graduate faster in times of higher college-educated unemployment if their family's income is also constrained at such times. For men in family income quintiles 1-3, the total marginal effect of a percentage point increase in post-college unemployment is 1.1 months delay of graduation; for women, it is 0.6 months delay of graduation.

Total student aid – Appendix Table A3 reports results from interacting college-educated unemployment rates with total freshman student aid. While student aid is

[30]The professional and vocation technologies category includes majors such as law enforcement/security, communications, architecture, and construction management.

insignificant in the main analysis of Equation (1.1) after accounting for family income, it is significant when interacted with post-college unemployment. Interaction coefficients for both genders are positive, indicating that students' unemployment-related delay increases with aid. While aid manipulation may tempt school administrators and aid policy makers as a tool to combat unemployment-related graduation delay, a very large reduction would be required to produce meaningful change in student behavior. Overall, men receiving total aid in the 25th percentile of generosity (approximately $6,400/year) delay 0.9 months for each increase of 1 percentage point in post-college unemployment, compared to delay of 1.0 months for men in the 75th percentile of freshman aid generosity ($13,600). Effects for women are approximately half as large as those for men. Tuition interactions display a similar relationship, with tuitions in the lowest quintile inducing students to stay in school longer during times of higher unemployment.

Small school status – Finally, Appendix Table A4 reports results from interacting college-educated unemployment rates with an indicator for whether the student attends a college in the lowest quintile of total enrollment. The average student body in such schools numbers 1,665 students. Results show that student's unemployment-related delay is magnified by attending a small school; affects are particularly pronounced for women. Small schools differ from others in many respects: 92% are private (compared with 35% of remaining schools), approximately 60% are religiously affiliated[31] (compared with approximately 17% of remaining schools), and they have more female students. Tuition at small schools is also considerably higher, and students are more likely to come from

[31] Data on religious affiliation is not available in the 2003 wave of the BPS.

out-of-state. Students at very small schools may have different personal networks, or differ in other unobserved ways from students at larger schools, which impact their job market prospects in a manner exacerbated by job market weakness.

Summary of interaction results – Overall, interaction results demonstrate that students respond to financial incentives by remaining longer in college when costs of study are reduced, or when the student's budget is less affected by negative unemployment shocks in the parent's employment market. Student delay behavior also differs significantly by freshman major. Unemployment-related delay shows no evidence of correlation with student ability as measured by high school GPA or SAT score, the degree of college location urbanity, student job market participation while in school, the public/private status of the college, whether the student attends school out-of-state, whether either parent holds a BA, or student race.

1.5 Conclusion

If the negative effects of graduating from college during a recession are long-lasting or permanent, students seeking to maximize their lifetime earnings may delay job market entry hoping conditions improve. However, little research to date has explored whether college students respond strategically to the job market they expect after graduation. I investigate whether college students strategically delay graduation in response to poor labor market conditions. I find significant evidence of strategic delay behavior associated with unemployment rates observed during the third year of study.

The main results indicate that the typical male college student delays graduation by between 0.48 and 0.88 months for each percentage point increase in unemployment for young college-educated workers observed during the junior year; the typical female student delays graduation by between 0.41 and 0.48 months for a similar change. Students' time to graduation is sensitive to job market shocks impacting the parent's budget constraint as well. In a recession characterized by a 4 percentage point increase in unemployment, these results imply the average student delays graduation by nearly half a semester. Students in certain majors delay even more as job market conditions deteriorate. For men, delays are most pronounced among those with freshman majors of humanities, life and physical sciences, education, business, and professional and vocational technologies. Among women, those beginning as undeclared, in the life and physical sciences, and in education delay in a manner correlated with junior year unemployment rates.

Interactions demonstrate that students respond to financial incentives by remaining longer in college when costs of study are reduced, or when the student's budget is less affected by negative unemployment shocks in the parent's employment market. Unemployment-related delay shows no evidence of correlation with student ability as measured by high school GPA or SAT score, the degree of college location urbanity, student job market participation while in school, the public/private status of the college, whether the student attends school out-of-state, whether either parent holds a BA, or student race.

Beyond simple delay of graduation, college students may respond to a poor job market by adding a second major, changing their major of study, or attending graduate school in hopes of improving their job market appeal. Existing evidence on whether students strategically change their subject of study or graduate school plans is scarce. Among undergraduates, Beffy, Fougere and Maurel (2009) find that while expected earnings hold some sway over student major choice, the effect is quantitatively small; students allocate greater weight to their perceived level of success in subject coursework, general ability, and personal interests than to expected returns when choosing a college major. In contrast, expected returns have a large impact on enrollment in grad school (Handa and Skolnik 1975). The effects of exiting into a recession on graduate school attendance are ambiguous. If recessions reduce the return to human capital, an investment in grad school may not pay off. But if graduate school allows a "do-over" entry into the job market, then riding out the downturn in school may prove worthwhile.

Chapter 2

Did Over-The-Counter Access to Emergency Contraception Increase Women's Educational Attainment?

Early childbearing is associated with low levels of educational attainment, workforce participation, low marriage rates, and poverty.[32] While these outcomes often stem from conditions existing prior to the decision to embrace motherhood,[33] it remains that attaining educational goals while caring for a young child is difficult and frequently impossible (Goldrick-Rab and Sorensen, 2010; Hofferth, Reid and Mott, 2001).

Approximately 5% of American women of reproductive age experience an unintended pregnancy annually, indicating a significant unmet need for contraception. Furthermore, women aged 24 years old and younger—the period when individuals make their largest investments in formal education—are the most likely among all reproductively able women to experience unintended pregnancy (Finer and Zolna, 2011). Mulligan (2012) shows that increased access to emergency contraception significantly

[32] A large literature documents these effects. See Hoffman and Maynard (2008) for a summary.

[33] Kearney and Levine (2007) suggest that the strong intergenerational correlation between economic disadvantage and early childbearing may result primarily from cultural norms; their later work (2012) suggests that young women with poor economic prospects choose early motherhood as they have no incentive to delay.

reduces the number of births for American women aged 20-29. Given the importance of educational investments made in young adulthood, does over-the-counter availability of emergency contraception increase women's likelihood of graduating from high school or college?

This study utilizes state-level variation in the regulation of emergency contraception (EC) to estimate the plausible causal relationship between of over-the-counter access to emergency birth control and the likelihood a woman completes high school, an associate's degree, or a bachelor's degree. I find that cohorts with greater access to EC are more likely to graduate from high school and attain the associate's degree; results for bachelor's degree attainment are also positive, but small. Effects for high school graduation are most pronounced among black women, while increases in associate's degree attainment are driven primarily by white and Hispanic women.

2.1 Institutional Background and Literature

Heated political debate over access to EC has fulminated since its introduction.[34] In 1998, Preven became the first "morning after" drug approved for use in the United States, followed by Plan B in 1999. Despite little medical indication for restriction, both drugs were available by prescription only until 2006, when the FDA approved Plan B for sale behind the pharmacy counter to women aged 18 and older without a prescription.

[34]Mulligan (2012) describes the political and legal environment around FDA approval for over-the-counter emergency contraception, from the mid-1990s to nation-wide OTC approval for women aged 18 and over in 2006.

The FDA lowered the legal age for purchasing Plan B to 17 in 2009, and expanded access to all women of reproductive age in June of 2013.

In the face of political wrangling on the national level, 9 states adopted legislation allowing pharmacists to prescribe and dispense EC directly to all women of reproductive age without a physician visit, between 1998 and 2006.[35] Women in states with pharmacist-access laws effectively experienced over-the-counter availability of EC prior to national approval. Because the efficacy of EC depends crucially on taking the medication within 72 hours of intercourse, requiring a physician visit severely limited women's effective access in states which did not adopt pharmacist-prescribing laws. As EC was available nationwide to women of all ages by doctor's prescription beginning with FDA approval of Preven in 1998, state legislation allowing pharmacists to prescribe effectively gave women younger than 18 over-the-counter (OTC) access legally equivalent to that of older women. In late February 1998, Washington state became the first to allow OTC access via its Emergency Contraception Pharmacist Pilot Project (PATH project). In the first four months of the program, participating pharmacists wrote and filled an average of 61 prescriptions per week, compared to 1 prescription per week before the program (Long and Fairfield, 1998). Other states took notice of the robust response to Washington's liberalization. Table 2.1 lists the 9 states adopting pharmacist access, and the year the statutes went into effect. As Mulligan (2012) notes, opposition to OTC treatment for EC is often associated with the mistaken view that EC causes medical

[35] Because EC was available nationwide to women of all ages by doctor's prescription beginning with FDA approval of Preven in 1998, state legislation allowing pharmacists to prescribe EC effectively gave women younger than 18 OTC access legally equal to that of older women.

Table 2.1: Emergency Contraception Legislation By State

State	Year in Effect
Washington	1998
California	2002
Alaska	2003
Hawaii	2003
New Mexico	2003
Maine	2004
Massachusetts	2005
New Hampshire	2006
Vermont	2006
All other states (FDA mandate)	2006

Source: Adapted from Mulligan (2012)

abortion. Despite the obvious political nature of the debate, Mulligan (2012) provides evidence that early state adoption of OTC status is not determined by the percentage of voters preferring Bush in 2000, whether a state mandates contraception coverage in health insurance, the percentage of the total population with a high school diploma or college degree, or trends and averages of abortion rates, birth rates, and STD infection rates.[36] However, because identification of the effect of OTC access to EC depends on state variation in legislation, concern nonetheless arises that states which passed legislation allowing pharmacist OTC access may differ in a manner correlated with educational outcomes, introducing bias. Section II, which describes the data, also

[36] See Mulligan (2012), Table 3.5.

presents checks of the exogeneity of OTC legislation and trends and averages in educational attainment and related factors.

This is the first paper to study the impact of EC access on women's educational outcomes. Prior work on the impact of EC has shown that increased access is associated with later marriage, lower single-mother birth rates, fewer births overall, an increase in risky sexual behavior, and higher rates of sexually transmitted infection (Mulligan, 2012; Zuppann, 2012). However, a large literature focuses on how other forms of birth control have changed women's educational choices and paid work. For example, Goldin and Katz (2002) find that the introduction of daily oral contraceptives ("the pill") increased women's educational attainment and workforce participation by lowering the cost of delaying marriage. Using plausibly exogenous variation in contraceptive access driven by state age of majority laws, Bailey (2006) finds that the pill decreased the likelihood a woman had her first child by age 22, thereby increasing workforce participation and hours worked. Additionally, Hock (2007) finds the pill increased college attainment, primarily by decreasing the likelihood a young woman dropped out of college due to pregnancy. These results suggests that expanding birth control options to women not utilizing long-term contraceptive measures should have a similar effect, increasing women's educational attainment and financial independence.

2.2 Empirical Strategy

This study uses state-level variation in over-the-counter (OTC) access to emergency contraception (EC) to estimate the effect of increased access on women's

educational outcomes.[37] For college outcomes, the main results use variation in access from both the pre-FDA states and the 2006 FDA mandate states; results for college outcomes using variation from the pre-FDA states only is presented as a robustness check. For high school completion outcomes, pre-FDA state variation is the most relevant, as the 2006 national OTC expansion did not cover those less than 18 years of age.[38,39]

The complete base model is

$$Y_{isc} = \alpha + \beta OTC_{isc} + X_{isc}\gamma + \theta_s + \eta_c + \phi_{sc} + \varepsilon_{isc} \quad (2.1)$$

where *s* indexes birth state, and *c* indexes year of birth (cohort). Educational outcome Y_{isc} for individual *i* alternately indicates whether the subject has completed high school, the associate's degree, or the bachelor's degree. OTC_{isc} indicates whether an individual had legal over-the-counter access to EC at or before age 24. As noted by Card (1999), few individuals are enrolled in school after their mid-twenties. Covariate matrix X_{isc} includes individual and state-by-cohort controls such as race/ethnicity, age at time of survey, and birth state median income, unemployment, and public education spending, which may be associated with outcomes Y_{isc} but unrelated to EC access. θ_s and η_c represent birth state

[37] Mulligan (2012) and Zuppann (2012) also use state-level variation in OTC access to EC, to examine the impact of access on sexual behavior, marriage, child birth, abortion, and STD infection.

[38] The FDA lowered the mandated OTC age to 17 in 2009, and eliminated the OTC age restriction entirely in 2013. However, women of all ages residing in pre-FDA mandate states could obtain OTC EC following the legislative action in their state; state pharmacy access laws superseded the federal mandate.

[39] Due to data limitations, all sample individuals born in states subject to the 2006 FDA mandate were aged 20 and older at the time EC became available in pharmacies without prescription. It follows that for ages when EC access has greatest likelihood of impacting high school completion outcomes, the data support pre-FDA state variation only. Many aspects of this analysis would be enhanced by additional treatment group observations, which will be possible as additional data become available. The overall impact of this data limitation on the results for high school completion is probably small; while women younger than 18 could obtain EC from a friend of legal age in the 2006 FDA mandate states, it seems unlikely this would occur with great enough frequency to bias the results.

and cohort fixed effects, respectively, and ϕ_{sc} represents birth-state specific linear time trends.[40] I include or exclude various portions of Equation (2.1) to observe the impact on the coefficient of interest in a sensitivity analysis excercise. In some specifications, OTC_{isc} is interacted with individual characteristics to explore differential effects among subgroups. For the binary outcome models utilized in this paper, I present coefficients from linear probability estimation; marginal effects from probit and logit models are analogous to the LPM results. For all analyses, I present heteroskedasticity-robust standard errors clustered at the birth state-by-birth cohort level.

Because the impact of EC access may vary by exposure intensity, I also consider

$$Y_{isc} = \alpha + \beta EXP_{isc} + X_{sc}\gamma + \theta_s + \eta_c + \phi_{sc} + \varepsilon_{isc} \quad (2.2)$$

where $OTCage_{isc}$ equals the earliest age those in individual *i*'s cohort *c* and birth state *s* could purchase EC legally without a doctor's prescription. Thus, $EXP_{isc} = \max\{24 - OTCage_{isc}, 0\}$ equals the number of years individual *i* enjoyed OTC access to EC before age 24. For example, those born in California in 1980 would have OTC access beginning in 2002 at age 22; for this individual, $EXP_{isc} = 2$. Therefore, EXP_{isc} captures the intensity of EC availability during the schooling years. Other variables are as in Equation (2.1) above. Equation (2.2) implies that the impact of an additional year of EC access is linear; I test this assumption by considering a nonparametric specification.[41] However,

[40] Another possible estimation strategy would use a multinomial model of college attainment, conditional on high school completion. However, this method introduces unnecessary selection bias, as the results of Equation (2.1) demonstrate that the likelihood of high school graduation is impacted by EC availability.

[41] Figures 2.1, 2.2 and 2.3 present the results of a similar equation, $Y_{isc} = \alpha + \sum_{k \in K} \beta_k I_{OTCage_{isc}^k} + X_{sc}\gamma + \theta_s + \eta_c + \phi_{sc} + \varepsilon_{isc}$ where K equals the set of OTC EC access ages present in the data (excluding one to avoid collinearity). The indicators $I_{OTCage_{isc}^k}$ produce a set of nonparametric estimates of EC access beginning at each age. Due to the small number of observed individuals with legal access before age 20, I combine ages of earliest access in 3-year increments:

due to the relatively small number of individuals with OTC access as very young women, fully nonparametric estimates lack precision. In some specifications, EXP_{isc} is interacted with individual characteristics.

2.3 Data

I utilize individual-level data from the 2000 Census and the American Community Surveys from 2001 to 2011. These datasets represent the largest, most current publicly available data sets for the US population which include birth state. Educational attainment is reported as of the time of survey. I determine OTC treatment status based on birth year cohort and birth state. This assumes that an individual resides in her state of birth during her education, and that EC is not accessible across state lines. Individuals are included in the study sample if they are between the ages of 10 and 30 in the year EC becomes available without a doctor's prescription in their birth state, at least 24 years old at the time of survey, and born no later than 1975. These criteria balance the need to retain the maximum number of late-cohort observations (those most likely to have OTC access to EC during their educational years) against the need to capture individuals who have substantially completed their educations, while only including those with relatively comparable attainment horizons and instructional environments.[42]

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first access prior to age 15, at ages 15-17, at ages 18-20, at ages 21-23, at ages 24-26, and at 30 and older. Indicators for first access at age 27-29 are excluded.

[42] While the number of individuals obtaining additional education after their mid-twenties is small, patterns of late-life attainment may indeed be non-linear. For example, women who did not attain the BA before having children may be more likely to return to college after their youngest child has entered school full-time, or when their spouse's career is firmly established. These events become more likely after age 30. Therefore, there is reason to believe that attainment, especially among women, may be non-linear in age.

A total of 1,021,667 women make up the study sample, with an average of 219 persons per birth state-by-birth year cell. The average age in the first year of OTC access to EC is 25.7 years old. An individual is considered a high school graduate if her highest level of education is listed as a high school diploma, GED equivalent credential, or higher level of education. An individual is considered to have attained less than 1 year of college without degree, more than 1 year of college without degree, or an associate's degree if her attainment at the time of survey is coded as such; if her attainment is at the bachelor's level or higher, she is considered to have obtained the bachelor's degree. Therefore, bachelor's degree attainment and high school graduation are not absorbing states.

Descriptive statistics and sample size details are presented in Table 2.2, for the total sample, by race/ethnicity, and by OTC legislation status at age 24. Overall, 93% of women obtain the high school credential, 7% attend college for less than 1 year without obtaining a degree, 18% attend college for a year or longer without obtaining a degree, 10% obtain an associate's degree, and 36% achieve a bachelor's degree. It is unknown whether those reporting some college but no degree were enrolled towards an associate's degree or a bachelor's degree; however, inclusion in these groups indicates the degree of investment the individual made in her education beyond high school. Analysis of all levels of post-high school attainment is important as it is unknown what attainment margin may be most impacted by the introduction of OTC EC access.

For both the pre-FDA states and the 2006 FDA mandate states, differences in mean attainment between the pre-OTC and post-OTC periods are evident. For the pre-FDA states, attainment of both at least one year of college without degree and the

Table 2.2: Descriptive Statistics Overall, by Race, and by EC Availability By Age 24

	All States				All Individuals: Pre-FDA States			All Individuals: 2006 Mandate States		
	All Individuals	Whites	Blacks	Hispanics	OTC Access by age 24	OTC Access after age 24	Difference	OTC Access by age 24	OTC Access after age 24	Difference
N	1,021,667	793,917	127,721	100,029	109,249	74,913	--	240,313	597,192	--
High school attainment	0.93	0.94	0.87	0.86	0.93	0.92	0.00**	0.93	0.93	0.00***
	(0.26)	(0.23)	(0.33)	(0.35)	(0.26)	(0.26)	(0.00)	(0.26)	(0.26)	(0.00)
Less than 1 year of college	0.07	0.07	0.08	0.08	0.08	0.08	0.00	0.07	0.07	0.00***
	(0.26)	(0.26)	(0.27)	(0.28)	(0.27)	(0.27)	(0.00)	(0.26)	(0.26)	(0.00)
At least 1 year of college	0.18	0.17	0.22	0.21	0.20	0.18	-0.02***	0.18	0.17	-0.01***
	(0.38)	(0.37)	(0.42)	(0.4)	(0.4)	(0.39)	(0.00)	(0.39)	(0.38)	(0.00)
Associate's degree	0.10	0.10	0.08	0.09	0.10	0.09	-0.01***	0.10	0.10	0.00
	(0.3)	(0.3)	(0.27)	(0.29)	(0.3)	(0.29)	(0.00)	(0.3)	(0.3)	(0.00)
Bachelor's degree	0.36	0.40	0.21	0.21	0.34	0.36	0.02***	0.37	0.36	-0.01***
	(0.48)	(0.49)	(0.41)	(0.41)	(0.47)	(0.48)	(0.00)	(0.48)	(0.48)	(0.00)
Age at OTC liberalization	25.67	25.77	25.96	24.52	20.50	26.77	6.27***	22.34	27.82	5.48***
	(3.4)	(3.36)	(3.23)	(3.66)	(2.71)	(1.43)	(0.01)	(1.49)	(1.76)	(0.00)
Age at survey	27.74	27.75	27.70	27.71	27.12	29.01	1.9***	25.69	28.52	2.83***
	(3.03)	(3.03)	(3.04)	(3.07)	(2.56)	(3.69)	(0.01)	(1.49)	(3.06)	(0.01)
OTC EC access at/before 24	0.34	0.33	0.32	0.46	1.00	0.00	--	1.00	0.00	--
	(0.47)	(0.47)	(0.46)	(0.5)	--	--	--	--	--	--
Years OTC EC access by 25	1.11	1.05	0.94	1.73	4.50	--	--	2.66	--	--
	(1.98)	(1.93)	(1.74)	(2.46)	(2.71)	--	--	(1.49)	--	--

Table 2.2 continues on the following page.

Table 2.2, Continued: Descriptive Statistics Overall, by Race, and by EC Availability By Age 24

	All States				All Individuals: Pre-FDA States			All Individuals: 2006 Mandate States		
	All Individuals	Whites	Blacks	Hispanics	OTC Access by age 24	OTC Access after age 24	Difference	OTC Access by age 24	OTC Access after age 24	Difference
White	0.78	1.00	--	--	0.68	0.73	0.05***	0.79	0.80	0.01***
	(0.42)	--	--	--	(0.47)	(0.44)	(0.00)	(0.41)	(0.4)	(0)
Black	0.13	--	1.00	--	0.06	0.06	0.00***	0.14	0.14	0.00
	(0.33)	--	--	--	(0.24)	(0.23)	(0.00)	(0.35)	(0.35)	(0.00)
Hispanic	0.10	--	--	1.00	0.25	0.21	-0.04***	0.08	0.06	-0.01***
	(0.3)	--	--	--	(0.44)	(0.41)	(0.00)	(0.26)	(0.24)	(0.00)
English not home lang.	0.09	0.04	0.03	0.60	0.19	0.17	-0.02***	0.08	0.07	-0.01***
	(0.29)	(0.19)	(0.18)	(0.49)	(0.39)	(0.37)	(0.00)	(0.27)	(0.26)	(0.00)
State K-12 spend	8,496.49	8,546.06	8,390.17	8,235.57	8,112.88	8,506.81	393.93***	9,036.90	8,293.23	-743.66***
	(1910.87)	(1876.42)	(2134.57)	(1850.7)	(1196.01)	(1135.73)	(8.06)	(1971.41)	(1988.05)	(5.02)
Higher education spend	371.64	369.19	376.31	385.22	450.99	263.71	-187.28***	472.40	330.09	-142.31***
	(108.05)	(106.75)	(107.4)	(117.5)	(118.07)	(69.8)	(0.48)	(87.46)	(72.75)	(0.19)
State median income	55,233.33	55,749.04	53,584.90	53,245.00	56,357.01	54,166.75	-2190.26***	57,691.06	54,172.55	-3518.51***
	(7728.31)	(7739.38)	(8111.47)	(6409.76)	(5949.23)	(7779.18)	(32.04)	(8092.02)	(7604.9)	(18.72)
State unemployment	5.08	4.99	5.15	5.75	5.64	6.76	1.13***	5.30	4.68	-0.62***
	(1.22)	(1.2)	(1.13)	(1.24)	(0.9)	(1.75)	(0.01)	(0.92)	(1.04)	(0.00)

Notes: Means are unweighted. Table includes women aged 24 years and older at the time of survey, between the ages of 10 and 30 in the year EC attained OTC status in her birth state, and born no later than 1975. Standard deviations in parenthesis. Standard errors presented for differences. OTC access by age 24 determined by birth year and state cohort. Individuals are considered Hispanic if they were coded as race "white" or "other" and ethnicity Hispanic. Individuals not categorized as white, black or Hispanic are excluded. K-12 total state spending on education at age 16 is in real CPI-adjusted dollars. Higher education spending at age 19 is in real dollars, adjusted using the Higher Education Price Index. State median household income, presented here at age 19, was calculated by the author using the March CPS from 1990-2011, code available upon request. Unemployment (here at age 19) was obtained from the BLS.
Significance indicated by --*: P<0.10, --**: P<0.05, --***: P<0.01.

associate's degree decreases in the post-OTC period, while bachelor's degree attainment increases. Among the 2006 FDA mandate states, statistically significant decreases in means are present for attainment of at least one year of college without degree and the bachelor's degree.

As expected, the mean age of those older than 24 when their birth state adopts OTC status for EC is higher than those in the treatment group, both the time of liberalization and at the time of survey. As educational attainment is increasing in age, controlling for age at the time of survey is important to proper identification. Additionally, in both pre-FDA and FDA states, treated individuals are more likely to be Hispanic and more likely to speak a language other than English at home, reflective of the changing youth demographic. This difference is larger in the pre-FDA states, which include two states with large Hispanic populations, California and New Mexico. The final portion of Table 2.2 describes state spending for K-12 and higher education, median income, and unemployment, each of which may impact educational attainment. Financial variables are stated in real dollars: K-12 spending per pupil is adjusted using the CPI, and public higher education spending per capita is adjusted using the Higher Education Price Index.[43] As median income and higher education spending are higher for treated individuals, these variables are included in the regression analysis to avoid omitted variables bias.[44]

[43]Obtained from the 2013 Higher Education Price Index report published by the Commonfund Institute.

[44]State K-12 spending at age 16 and state median income at age 16 are not significant in regressions of high school graduation and is therefore excluded from the final analysis. In assessing college outcomes, only state median income at age 19 and birth state unemployment at age are significant and included in the final analysis.

Because identification depends on state variation in legislation, the results will only be free of bias if early-adopter states do not differ from other states in a manner correlated with educational outcomes. As with any analysis dependent on cross-state variation, there is risk that a state-level shock in a given year could bias the results. In an effort to assess this risk, Table 2.3 presents the results of endogeneity checks for correlation between pre-FDA mandate OTC legislation, educational attainment rates, and birth state characteristics which may be correlated with attainment including spending on K-12 education, spending on higher education, unemployment rates, median income, and whether English is the primary language spoken in the individual's home.[45,46] Coefficients in Table 2.3 are produced by OLS regression of an indicator for pre-FDA OTC status on either the birth state trend or average variable of interest. For the educational attainment covariates, the only statistically significant relationship observed for cohorts aged 21 and older by 2001 is for associate's degree attainment, where passage of OTC legislation is correlated with decreasing attainment. If anything, this would tend to bias the analysis herein against any finding that EC access has increased associate's degree attainment. None of the average levels of educational attainment are correlated with pre-FDA OTC legislation.

[45]Data on total and instructional spending (per pupil) for primary and secondary education for the 50 states and the District of Columbia are obtained from U.S. Census Bureau annual reports on public elementary and secondary education finances from 1993 to 2011. Data on state spending for higher education for the 50 US states are obtained from the annual appropriations report compiled by the Center for Higher Education at Illinois State University, from 1994 to 2009. K-12 state spending is stated in real CPI-adjusted dollars. Higher education spending stated in real dollars, adjusted using the Higher Education Price Index. Data on unemployment was obtained from the BLS.

[46]K-12 educational spending is for the year an individual is age 16. Spending on higher education, unemployment rates, and median income are for the year an individual is age 19.

Table 2.3: Education Spending, Attainment Rates, and the Presence of OTC Legislation

Dependent variable = 1 if state passed legislation granting pharmacy access to EC prior to the FDA ruling.

	Trend		Average	
Independent variable	Estimate	Std. Error	Estimate	Std. Error
Educational attainment				
High school attainment	-6.014	(19.526)	1.157	(2.488)
Less than 1 year of college	8.132	(23.297)	3.354	(4.587)
At least 1 year of college	17.193	(12.120)	-0.866	(2.174)
Associate's degree	-43.467**	(16.653)	-3.191	(2.393)
Bachelor's degree	-4.809	(10.743)	0.388	(0.812)
Birth state characteristics				
K-12 spending	-0.001	(0.000)	0.000	(0.000)
Higher education spending	-0.015**	(0.006)	-0.000	(0.000)
Unemployment	-0.423	(0.295)	0.018	(0.062)
Median income	-0.000**	(0.000)	0.000	(0.000)
English not home language	24.910*	(13.098)	2.521**	(0.981)

Notes: Table presents OLS results for regressing an indicator for pre-FDA OTC status on the indicated linear trend or average independent variable and a constant term. Trends and averages calculated using data for individuals aged 21 and older by 2001. Results for aged 21 and older by 1999 and 2003 are similar. K-12 state spending in real CPI-adjusted dollars. Higher education spending in real dollars, adjusted using the Higher Education Price Index. Significance indicated by --*: P<0.10, --**: P<0.05, --***: P<0.01.

Table 2.3 also presents coefficients for various birth state characteristics. In addition to the possibility of direct correlation between state high school and college attainment rates and education funding, changes in funding may indicate other state-level changes to the structure of the education system relevant to completion. Table 2.3 shows that early adoption of OTC status for EC is not dependent on the trend or average of

overall or instructional funding for K-12 education. For higher education funding, early OTC status for EC is associated with decreasing state appropriations in the trend, but not on average. As it seems unlikely that state cuts in higher education spending would enable more students to graduate, this correlation will tend to bias my analysis against any finding that EC access improves graduation rates. Likewise, the positive correlation noted between pre-FDA OTC access and non-English speaking homes will dampen any results; it is unlikely that lower English proficiency will confer a completion advantage in an English-dominant education system.

2.4 Results

In Tables 2.4-2.6, variation is derived from both the pre-FDA states and the 2006 FDA mandate states; as a robustness check, the final column of each table presents pre-FDA status interactions. Because the 2006 FDA mandate did not extend OTC access to minors, while the pre-FDA state legislation did, variation derived from pre-FDA states only is more relevant to high school completion than is variation from the pre-FDA states plus the mandate states. However, as women younger than 18 in the mandate states could obtain EC from a friend of legal age, whether the full-variation specification is relevant to high school completion is open to empirical discovery.

2.4.1 Impact of OTC EC Exposure At or Before Age 24

Several specifications of Equation (2.1) are reported in Table 2.4. Panel A presents results for the binary outcome of high school completion. Column (1) contains the unadjusted different-in-difference estimates, which are not statistically significant. Adding indicators for race/ethnicity in three categories and whether a language other than English is dominant in the individual's household more than doubles the magnitude of the estimated effect of EC access, indicating the possibility of differential impact of access by race or language status. Controlling for age at the time of survey in column (3) again more than doubles the coefficients, indicating that although high school completion after age 24 is unlikely, it occurs frequently enough to bias the results if excluded. However, after adjusting for state and cohort fixed effects in column (4), the estimate is greatly reduced in magnitude and no longer rises to the level of statistical significance. Controlling for birth state unemployment rates at age 16, allowing effect of unemployment to vary by birth year, and adjusting for state-linear time trends does little to alter the coefficient. However, as column (8) shows, interacting OTC access with pre-FDA status produces a positive and significant interaction term. As noted above, pre-FDA variation is the most relevant for the high-school aged population during the years covered by the data; this result suggests that the OTC minimum purchase age of 18 years old in the 2006 mandate states was binding enough to keep EC from having a significant impact on educational outcomes for minors. In states with pharmacist access laws, however, OTC access increased the likelihood of high school completion. Given a baseline graduation rate of 93% by age 25, this implies that legal OTC access to EC at or

Table 2.4: Effect of Over-The-Counter Emergency Contraception Availability by Age 24 on Women's Education Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: High school graduation								
OTC access by age 24	0.003	0.007***	0.015***	0.002	0.002	0.001	0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
OTC access by age 24 X pre-FDA state								0.009**
								(0.004)
Race and language controls		X	X	X	X	X	X	X
Age at time of survey			X	X	X	X	X	X
Birth state and cohort fixed effects				X	X	X	X	X
State unemployment					X		X	X
Allowing effect of unemployment to vary by birth year						X		
State-linear time trend							X	X
Panel B: Associate's degree attainment								
OTC access by age 24	0.001	0.001	0.008***	0.004**	0.004**	0.003	0.004***	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)
OTC access by age 24 X pre-FDA state								0.004
								(0.004)
Race and language controls		X	X	X	X	X	X	X
Age at time of survey			X	X	X	X	X	X
Birth state and cohort fixed effects				X	X	X	X	X
State median income and unemployment					X		X	X
Allowing effect of median income and unemployment to vary by birth year						X		
State-linear time trend							X	X

Table 2.4 continues on the following page.

Table 2.4, Continued: Effect of Over-The-Counter Emergency Contraception Availability by Age 24 on Women's Education Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel C: Bachelor's degree attainment								
OTC access by age 24	-0.003	0.004	0.026***	0.004	0.005	0.006**	0.002	0.005
	(0.007)	(0.006)	(0.007)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
OTC access by age 24 X pre-FDA state								-0.006
								(0.007)
Race and language controls		X	X	X	X	X	X	X
Age at time of survey			X	X	X	X	X	X
Birth state and cohort fixed effects				X	X	X	X	X
State median income and unemployment					X		X	X
Allowing effect of median income and unemployment to vary by birth year						X		
State-linear time trend							X	X

Notes: Dependent variable equals 1 if the individual has the indicated educational attainment. The variable of interest is an indicator for birth state OTC access to EC at or prior to age 24. Within each panel, each column represents a separate regression. "Race and language controls" include indicators for black and Hispanic race/ethnicity, and whether English is the primary language spoken in the individual's home. For high school outcomes, state unemployment is measured at age 16. For college outcomes, state unemployment and state median income are measured at age 19. Standard errors are clustered at the birth state-by-birth year level.
Significance indicated by --*: P<0.10, --**: P<0.05, --***: P<0.01.

Table 2.5: Effect of Years Exposure to Over-The-Counter Emergency Contraception Prior to Age 24 on Women's Education Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: High school graduation								
Years OTC access X pre-FDA state	0.000	0.002***	0.003***	-0.000	-0.000	-0.000	-0.002	
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	
Race and language controls		X	X	X	X	X	X	
Age at time of survey			X	X	X	X	X	
Birth state and cohort fixed effects				X	X	X	X	
State unemployment					X		X	
Allowing effect of unemployment to vary by birth year						X		
State-linear time trend							X	
Panel B: Associate's degree attainment								
Years of OTC access prior to age 24	0.000	0.000	0.002***	0.001*	0.001	0.001*	0.002*	0.002**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Years OTC access X pre-FDA state								-0.002*
								(0.001)
Race and language controls		X	X	X	X	X	X	X
Age at time of survey			X	X	X	X	X	X
Birth state and cohort fixed effects				X	X	X	X	X
State median income and unemployment					X		X	X
Allowing effect of median income and unemployment to vary by birth year						X		
State-linear time trend							X	X

Table 2.5 continues on the following page.

Table 2.5, Continued: Effect of Years Exposure to Over-The-Counter Emergency Contraception Prior to Age 24 on Women's Education Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel C: Bachelor's degree attainment								
Years of OTC access prior to age 24	-0.005***	-0.002*	0.002*	-0.004***	-0.003***	-0.003***	-0.002	-0.002
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Years OTC access X pre-FDA state								0.002
								(0.002)
Race and language controls		X	X	X	X	X	X	X
Age at time of survey			X	X	X	X	X	X
Birth state and cohort fixed effects				X	X	X	X	X
State median income and unemployment					X		X	X
Allowing effect of median income and unemployment to vary by birth year						X		
State-linear time trend							X	X

Notes: Dependent variable equals 1 if the individual has the indicated educational attainment. The variable of interest equals the number of years an individual had OTC access to EC before age 25. Within each panel, each column represents a separate regression. "Race and language controls" include indicators for black and Hispanic race/ethnicity, and whether English is the primary language spoken in the individual's home. For high school outcomes, variation is derived from the pre-FDA states only, and state unemployment is measured at age 16. For college outcomes, variation is derived from the pre-FDA states combined with the 2006 FDA mandate states, and state unemployment and state median income are measured at age 19. Standard errors are clustered at the birth state-by-birth year level.
Significance indicated by --*: P<0.10, --**: P<0.05, --***: P<0.01.

before age 24 resulted in a 0.8% increase in the high school attainment rate (baseline attainment rates are presented in Table 2.2).

Panel B of Table 2.4 presents results for Equation (2.1) for the outcome of associate's degree attainment. Like the high school completion analysis, controlling for race, home language and age at survey in column (3) significantly increases the estimated effect of EC, and controlling for birth state and cohort diminishes it. While state median income and unemployment at age 19 have little impact on the estimate, interacting these state-level variables with birth cohort in column (6) slightly reduces the estimate of EC's impact on attainment. While inclusion of state linear trends in column (7) triples the magnitude of the estimate, it is not robust to the interaction of OTC access with pre-FDA status in column (8). However, coefficients on both the main effect and the interaction term remain positive. Utilizing variation from all states combined, the coefficient in column (7) implies that the introduction of EC is associated with a 4% increase in the associate's degree attainment rate, relative to a baseline attainment rate of 10%.

Panel C of Table 2.4 presents results for Equation (2.1) for the outcome of bachelor's degree attainment. As with prior levels of attainment, isolating the effect of EC requires controlling for race, home language and age at survey. Also as before, the coefficient of interest is diminished after controlling for birth state and cohort fixed effects in column (4). However, unlike high school graduation and associate's degree attainment, introduction of state median income and unemployment at age 19 does impact the coefficient on EC access, and allowing the effect of these covariates to change with birth year causes the EC coefficient to rise to the level of significance (column [6]).

It may be that trends in student budget constraints for those attempting the bachelor's degree align with birth year in a manner which masks the overall effect of EC for these individuals. This interpretation is supported by the fact that controlling for state linear time trends in column (7) significantly reduces the EC coefficient. Although the coefficient on the interaction term in column (8) is insignificant, the fact that it is negative reflects the slightly negative trend in BA attainment associated with the pre-FDA states, noted in Table 2.3. In the presence of the interaction term, the coefficient on the main effect recovers in magnitude, but does not rise to statistical significance.

2.4.2 Impact of OTC EC Exposure Intensity Before Age 24

Table 2.5 reports the results of Equation (2.2), which considers how the intensity of EC exposure impacted educational outcomes; the coefficient of interest is years of EC access prior to age 24. We may expect coefficients in Table 2.5 to be of smaller magnitude than those in Table 2.4, as Equation (2.2) estimates the marginal impact of an addition year of OTC access on educational attainment, rather than the value of any exposure prior to age 24.

Panel A displays coefficients on EC access for the outcome of high school completion. Due to the finding in column (8) of Table 2.4 that only variation from the pre-FDA states matters to high school completion, high school completion results for Table 2.5 are derived from pre-FDA state variation only. As before, controlling for race and language in column (2) as well as age at survey in column (3) increases the estimated

effect of EC; however, controlling for birth state and cohort eliminates the estimated effect. Accounting for state time trends does not restore statistical significance.

Panel B of Table 2.5 presents results for Equation (2.2) for the outcome of associate's degree attainment. Controlling for age at survey in column (3) increases the estimated effect of EC and its precision; as in Table 2.4, controlling for birth state and cohort diminishes the estimated effect of EC access. As expected, the overall magnitude of the estimated effects is smaller than those in Table 2.4. After controlling for trends in columns (6) and (7), the coefficient regains significance; however, the pre-FDA interaction in column (8) indicates that, at least for early adopters, additional years of access have a negative impact on associate's degree completion. The negative interaction term in column (8) recalls the negative trend in associate's degree attainment correlated with early OTC adoption, as noted in Table 2.3. Thus, the marginal impact of an additional year or EC access on associate's degree attainment in pre-FDA states equals zero.

Panel C of Table 2.5 presents results of Equation (2.2) for the outcome of bachelor's degree attainment. The results differ markedly from those in Table 2.4. Counterintuitively, many coefficients are negative. After accounting for birth state and cohort fixed effects in column (4), the estimated effect of an additional year of OTC access to EC is negative and significant. However, controlling for state-linear trends in column (7) reveals that this effect is generally unrelated to EC access and confined to particular states; column (8) demonstrates that the states in question are not early adopters.

Another way to quantify the intensity effect of EC availability is to consider a nonparametric version of Equation (2.2) (see the detailed description in Footnote 41). As the results are difficult to interpret in numerical form, I present the coefficients graphically in Figures 2.1-2.3. The primary challenge to interpreting these results arises from the fact that very few individuals in the study sample are younger than 18 at the time they first obtain EC access, and none of those in 2006 FDA mandate states enter the treatment group prior to age 20. These data limitations result in estimates which are both imprecise and prone to bias, especially for the younger ages. As Figure 2.1 demonstrates, the data do not indicate a firm relationship between age at first EC access and high school completion; the dashed lines represent 95% confidence intervals. However, as high school completion generally occurs well before age 20, it follows that these results may not fully capture the true effect of EC; to do so will require data for later cohorts, which are currently unavailable. Figures 2.2 and 2.3 present results for associate's degree and bachelor's degree attainment, respectively. For associate's degree attainment, the overall relationship is positive; however, consistent with trends towards lower associate's attainment in pre-FDA states, estimates for access at early ages (where only pre-FDA observations are available) are negative. A more robust estimation of this relationship will become possible as more data are available. Figure 2.3, for bachelor's degree attainment, conforms most closely to the intuition that earlier EC access begins, the higher educational attainment should be, and reflects the lack of significant pre-existing BA attainment trends in early adopter states.

Figure 2.1: Impact of Age at EC Liberalization on High School Graduation

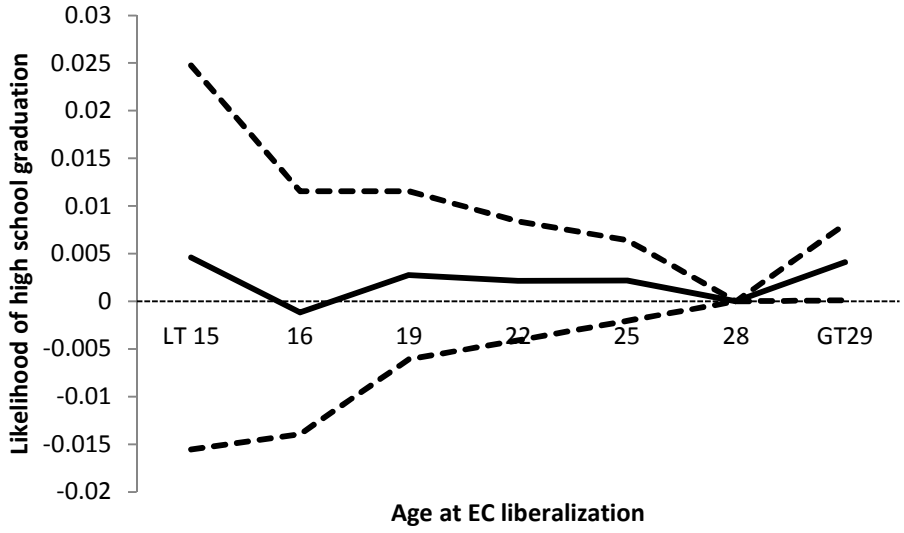


Figure 2.2: Impact of Age at EC Liberalization on Associate's Degree Attainment

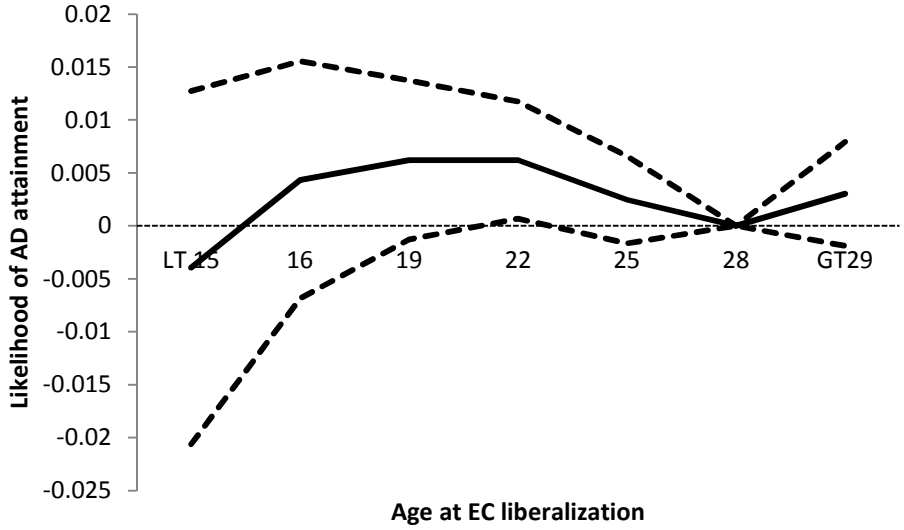
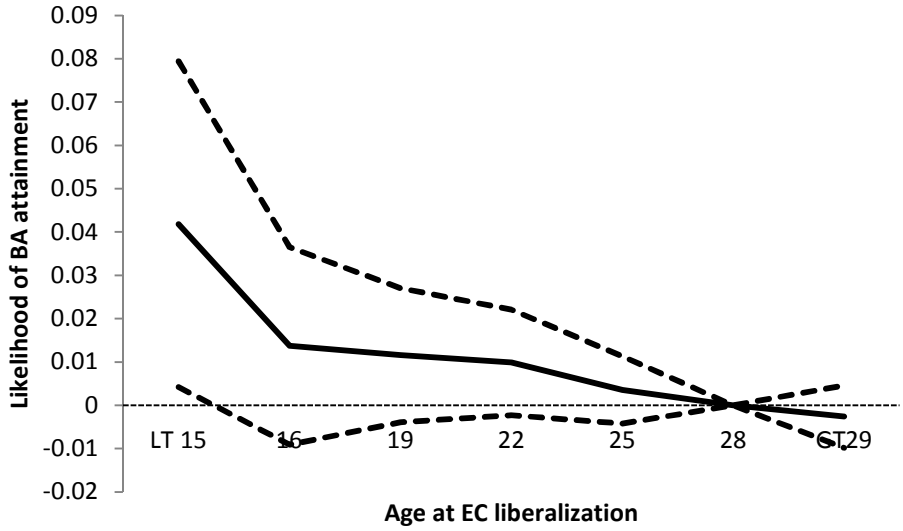


Figure 2.3: Impact of Age at EC Liberalization on Bachelor's Degree Attainment



2.4.3 Allowing the Effect of Over-The-Counter Emergency Contraception to Vary With Personal Characteristics

Of the specifications considered in Tables 2.4 and 2.5, specification (7) appears to best account for preexisting trends in attainment. Therefore, the interaction estimates presented in Table 2.6 include the covariates present in specification (7): controls for race, age at survey, birth state unemployment and (for college outcomes) median income, birth state and cohort fixed effects, and state-linear time trends. Table 2.6 includes estimates for both Equations 1 and 2. For reference, columns (1) and (4) repeat the non-interacted results from columns (7) of Tables 4 and 5, respectively. As only permanent personal characteristics are relevant for analysis, I present interactions of EC access with

race/ethnicity and whether the primary language spoken in the respondent's home is one other than English.

Overall, interaction estimates for Equations (1) and (2) are consistent with one another. Panel A presents results for the outcome of high school completion. Interacting with race, columns (2) and (5) reveal that black women were the primary beneficiaries of EC access in terms of high school attainment. In terms of marginal effects, granting EC access at or prior to age 24 results in a 2.1% increase in the likelihood a black woman will earn the high school credential and a 0.4% increase in the likelihood a white woman will do so (significant at the 1% and 5% levels, respectively). The marginal estimate for Hispanic women, equal to that for white women, is imprecise; note that excluding the non-English dummy does not impact the results, indicating that the presence of the non-English dummy is not swamping any marginal effect of Hispanic ethnicity. Interacting on English use at home in (columns [3] and [6]) produces significant and positive interaction terms for both models. The marginal effects indicate that women in non-English speaking homes with EC at or prior to age 24 are approximately 1.5% more likely to graduate from high school than those without access (significant at the 1% level). This large effect may reflect unobserved differences among ethnic and social groups (such as first-generation immigrants) in the consistent use of birth control.[47]

[47]As an example of how birth control practices vary by ethnic, cultural or religious identity, Daniels, Mosher and Jones (2013) report that Hispanic are 10% less likely than white women to have frequently used any highly effective, reversible birth control, and that Hispanic women are significantly less likely than white and black women to have had a partner use a condom during sex. They also report that Catholic women were less likely to use both condoms and the birth control pill to prevent pregnancy, and were more likely to engage in periodic abstinence (the "timing method") than those of other faiths or no faith.

Table 2.6: Allowing the Effect of Over-The-Counter Emergency Contraception to Vary With Personal Characteristics

	OTC Access by Age 24			Years OTC Access Before Age 24		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: High school graduation						
Pre-FDA OTC access	0.006**	0.005**	0.004	-0.002	-0.002*	-0.002*
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Pre-FDA OTC access X black		0.016***			0.003***	
		(0.003)			(0.001)	
Pre-FDA OTC access X Hispanic		-0.001			0.000	
		(0.004)			(0.001)	
Pre-FDA OTC access X non-English home			0.011**			0.003***
			(0.005)			(0.001)
Panel B: Associate's degree attainment						
OTC access	0.004***	0.005***	0.004***	0.002*	0.002**	0.002*
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
OTC access X black		-0.003			-0.002**	
		(0.002)			(0.001)	
OTC access X Hispanic		-0.001			-0.001	
		(0.002)			(0.001)	
OTC access X non-English home			-0.000			-0.000
			(0.003)			(0.001)
Panel C: Bachelor's degree attainment						
OTC access	0.002	0.005*	0.004	-0.002	-0.001	-0.002
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
OTC access X black		-0.009			-0.002	
		(0.006)			(0.001)	
OTC access X Hispanic		-0.015**			-0.003**	
		(0.007)			(0.001)	
OTC access X non-English home			-0.012*			-0.002
			(0.007)			(0.001)

Notes: Dependent variable equals 1 if the individual has the indicated educational attainment. In columns 1-3, the variable of interest is an indicator for birth state OTC access to EC at or prior to age 24. In columns 4-6, the variable of interest is the number of years an individual had OTC access to EC before age 24. Within each panel, each column represents a separate regression. Controls include state-linear trends, and indicators for black and Hispanic race/ethnicity, whether English is the primary language spoken in the individual's home, birth state, and cohort. For the outcome of high school graduation, variation is derived from the pre-FDA states only, and regressions include state unemployment at age 16. For college outcomes, variation is derived from the pre-FDA states combined with the 2006 FDA mandate states, and regressions include state unemployment and state median income at age 19. Standard errors are clustered at the birth state-by-birth year level.
Significance indicated by --*: P<0.10, --**: P<0.05, --***: P<0.01.

In contrast to the interaction results for high school completion, allowing the effect of EC access on associate's degree completion to vary with race and language characteristics is reveals somewhat weaker relationships. As shown in columns (2) and (5), associate's degree attainment by blacks and Hispanics is slightly negatively impacted by expanded EC access; the interaction coefficient for blacks rises to significance in column (5). These negative coefficients indicate that the overall positive impact of EC access on associate's degree attainment is attributable to increased completion among white and Hispanic women; marginal estimates of the impact of EC access on associate's degree completion indicate a 0.4% increase in attainment for both groups (significant at the 1% and 10% levels, respectively). The fact that white and Hispanic women are impacted while black women are not may reflect differences in the underlying marginal educational attainment opportunities of each group: what levels of attainment are most commonly within reach. Interactions of EC access with language status have no significant impact on junior college completion.

Panel C, which presents interaction results for bachelor's degree completion, follows the same general pattern as Panel B: negative interaction terms for minority groups and those in non-English speaking households indicate that the primary positive impact of EC access on BA attainment occurs among native-born whites. Columns (2) and (5) suggest that EC access actually decreases attainment for women of Hispanic descent, although the marginal effects are imprecisely estimated. For white women, access to EC by age 24 is associated with a marginal increase of 0.5% in graduation with the bachelor's degree (significant at the 10% level). Similarly, the interaction coefficient

for EC and home language status is negative and significant; however, as the main effect of coming from a non-English speaking home on BA completion is positive,[48] there is no net marginal impact.

2.5 Placebo Tests

Results for placebo tests are presented in Table 2.7 for two specifications: one regressing educational attainment on an indicator for first receiving OTC access to EC at age 25 or older (in the spirit of Equation [1]), and another regressing educational attainment on indicators for whether an individual's first year of OTC access occurs at ages 25-30 (similar to the estimation used to produce Figures 2.1-2.3). Aside from the impact of attainment trends correlated with early OTC adoption (see Table 2.3), and assuming that educational attainment after age 25 is limited, we expect that increased access to EC after age 25 will have little impact on educational attainment. However, in an era of expanding adult education this assumption may be untenable. Table 2.7 indicates that expanded access to EC after age 25 has no impact on high school graduation or bachelor's degree attainment. For associate's degree attainment, the results of specifications 1 and 2 seem contradictory: specification 1 implies that OTC access beginning at age 25 is associated with a decrease in attainment, while specification 2 produces positive and significant coefficients on indicators for first EC access at ages 28,

[48] While at first it seems counter-intuitive that women from non-English speaking homes would be at once less likely to graduate from high school and more likely to graduate from college, this reflects the bimodal nature of the U.S. immigration process. While the majority of immigrants arrive illegally from Spanish-speaking nations and come from families with generally low educational attainment, a large fraction of immigrants do come from educated families, who would naturally expect high attainment from their children and likely have the resources to enable that attainment.

Table 2.7: Placebo Tests

	High school		Associate's degree		Bachelor's degree	
	(1)	(2)	(1)	(2)	(1)	(2)
First OTC access at age 25+	-0.002		-0.004***		-0.002	
	(0.002)		(0.001)		(0.003)	
First OTC access at age 25		0.001		-0.000		-0.001
		(0.002)		(0.002)		(0.004)
First OTC access at age 26		-0.001		0.003		-0.007
		(0.003)		(0.003)		(0.005)
First OTC access at age 27		-0.002		0.002		-0.009
		(0.004)		(0.003)		(0.006)
First OTC access at age 28		-0.002		0.008**		-0.003
		(0.004)		(0.003)		(0.007)
First OTC access at age 29		0.003		0.012**		-0.008
		(0.006)		(0.005)		(0.010)
First OTC access at age 30		0.002		0.017***		-0.011
		(0.008)		(0.006)		(0.012)

Notes: Dependent variable equals 1 if the individual has the indicated educational attainment. Each column is a separate regression. LMP regressions include age at time of survey, indicators for black and Hispanic race/ethnicity, an indicator for whether English is the primary language spoken in the individual's home, birth state and birth cohort fixed effects, and state-specific linear trends. Regressions for high school graduation outcomes include birth state median income at age 16. Regressions for college outcomes include birth state median income at age 19, and birth state unemployment rate at age 19. Standard errors are clustered at the birth state-by-birth year level.
Significance indicated by --*: P<0.10, --**: P<0.05, --***: P<0.01.

29 and 30. As the data include a large number of untreated observations, which help to control for preexisting trends, and all regressions include both cohort and birth state fixed effects and linear state trends, these results raise the question of whether adequate variation is present to identify the effects of OTC access to EC. This matter cannot be settled definitively until more long-term data are available for the treated cohorts.

2.6 Conclusion

Unintended pregnancy is most common during women's primary years for educational investment. Does over-the-counter availability of emergency contraception (EC) increase women's likelihood of completing high school, an associate's degree, a bachelor's degree, or attaining some college training? This study utilizes state-level variation in the regulation of emergency contraception to estimate the plausible causal relationship between over-the-counter (OTC) access to EC and women's educational attainment.

Despite limitations in the available data, I find that cohorts with greater access to EC graduate from high school and attain the associate's degree with greater frequency. The effect of EC on bachelor's degree attainment is more muted. Over-the-counter access to EC prior to age 24 is associated with a 0.8% increase in high school completion the average woman; this effect is strongest among black women, who are 2.1% more likely to graduate when given OTC access. Associate's degree attainment is also positively related to EC access; estimates suggest that OTC EC access by age 24 lead to a 4% increase in the rate of associate's degree attainment (relative to a baseline attainment rate of 10%), primarily among white and Hispanic women. As the attainment levels most impacted by EC access are commonly achieved early in a woman's reproductive life, expanding OTC access minors (as the FDA recently did) seems an expedient and effective way to help at-risk young women climb one rung higher on the educational ladder.

That EC introduction has the greatest marginal impact at the low end of the educational attainment spectrum may reflect its status as a last-resort pregnancy prevention method. EC is used most frequently by those who are not already following a set birth control regimen. If women choose EC because health care access barriers prevent them from using a more reliable form of reversible birth control, such as the pill, then expanded health care access and mandated birth control coverage in insurance plans may dampen EC's impact on educational outcomes for future cohorts. Additional data on cohorts experiencing OTC access to EC as young women will help solidify the causal nature of this relationship, and further our understanding of how EC changes women's education and lives.

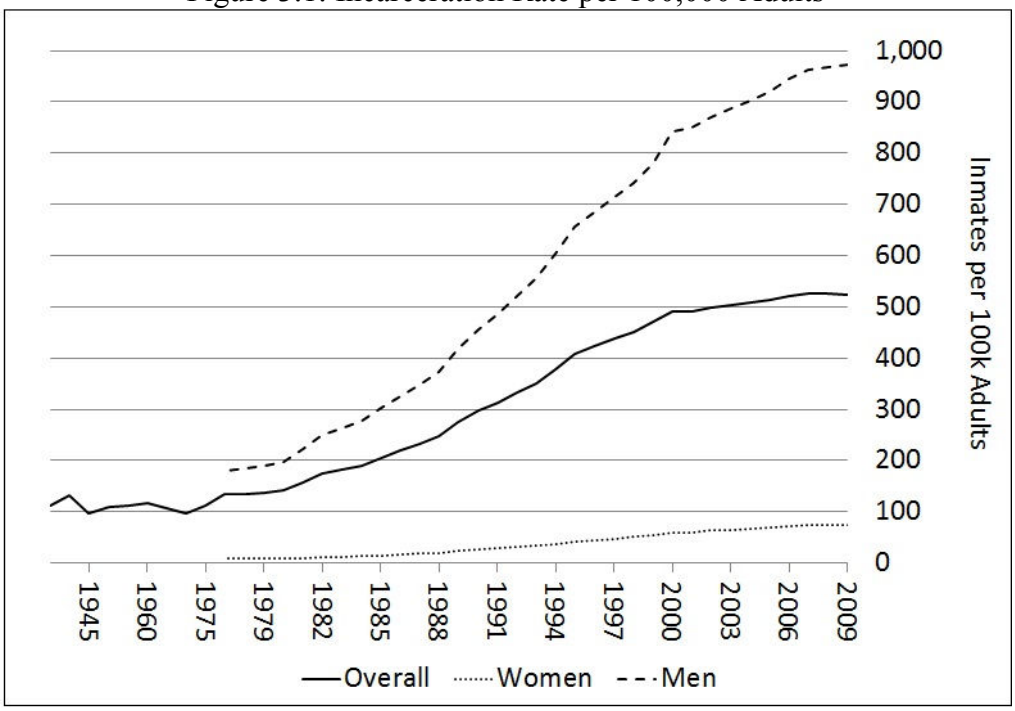
Chapter 3

Locked Up, Locked Out: Incarceration and Inequality in an OLG Economy with Race and Endogenous Education

From 1977 to 2009, the number of individuals incarcerated in US state and federal prisons increased by nearly five-fold, from under 290,000 in 1977 to over 1.6 million in 2009.[49] The explosion in prison populations over the past three decades is clear from Figure 3.1. America currently imprisons more people, both per capita and in total number, than any other country. Lifetime chances of incarceration for minorities and those with low educational attainment are particularly high. For example, Pettit, Sykes and Western (2009) estimate that black men born between 1975 and 1979 are estimated to face a 26.8% cumulative risk of imprisonment by age 34 (5.4% for whites), increasing to 68.0% for high school dropouts (28.0% for white dropouts). Western and Pettit (2010) further explore the link between educational attainment and imprisonment: from 1980 to 2008, incarceration rates for the college educated are almost unchanged, while rates for those with the least education have nearly quadrupled. As they note, going to prison is a

[49] These data, as well as those used to produce Figure 3.1, are compiled from the Bureau of Justice Statistics Prisoners Series and National Prisoner Statistics series, available upon request. Totals exclude those held in local jails, unless due to state or federal facility overcrowding.

Figure 3.1: Incarceration Rate per 100,000 Adults



Notes: Data from 1977 to 2009 are compiled from the Bureau of Justice Statistics Prisoners Series and National Prisoner Statistics series. Data prior to 1977 are from Austin et al. (2000) Figure 1.

normal life event for an entire generation of poorly educated young men. That incarceration occurs most frequently among those at the bottom of the socioeconomic ladder and traditionally disadvantaged groups makes it more likely to reinforce existing inequality; although blacks made up only 12% of the total US population in 2009, they represented over 38% of all prisoners.[50] Furthermore, data suggest that the surge in prison populations is not due to change in underlying individual behavior, but in crime policy: longer sentences and a more stringent parole environment (Blumstein and Beck, 1999).

[50]Calculated using data from the CPS report Labor Force Characteristics by Race and Ethnicity, 2009, and Bureau of Justice Statistics Prisoners in 2009 Appendix Table 12.

The negative impact of imprisonment on individuals and families is well-documented: ex-cons experience low wages, vocational stigma, high unemployment, family instability, recidivism, and restrictions on political and social rights.[51] As the resources available to a child significantly impact his ability to succeed as an adult, parental imprisonment may catalyze a cycle of poverty. Furthermore, the significant race and education disparities in America's prison population suggest that understanding the relationship between incarceration, race and education may be vital to understanding cycles of poverty and evaluating criminal policy. How much do longer prison sentences, and a higher likelihood of capture and conviction, contribute to income and asset inequality, both for society as a whole and between demographic groups? Using an OLG model with race, inheritance, and endogenous education choice, I find that an 18% increase in the criminal apprehension rate (proxied by the product of the crime clearance and conviction rates) and a 68% increase in prison sentence length together have little impact on inequality as measured by Gini coefficients for income and assets, as well as the income share accruing to blacks. Instead, the more stringent criminal policy markedly decreases crime and incarceration rates relative to the counter-factual exercise.

3.1 Related Literature

I build upon the economic literature of crime opened by Becker (1968) and expanded by many others. İmrohoroğlu, Merlo and Rupert (2004, 2006) explore a dynamic general equilibrium OLG model wherein agents chose each period whether to

[51] See Raphael (2010), and Pettit and Western (2004) for a discussion of the sociology literature.

commit a property crime. Agents committing a crime obtain a portion of the average agent's current-period earnings, and are apprehended with positive probability. Agents who are robbed suffer the loss of a portion of their specific current-period income. Agents differ ex-ante in their income earning abilities. İmrohoroğlu et al. (2004, 2006) calibrate the model to crime rates in 1980, taking as exogenous age demographics, and the distribution of human capital approximated by the portion of the population with less than high school, high school, college, and post-graduate education. They apply a life-cycle approach to the calibration of employed and unemployed incomes, and unemployment transition probabilities based on agent's human capital endowments. They abstract from the criminal justice system, in that all agents who are apprehended are incarcerated, and obtain a positive level of crime in equilibrium. Their model approximates the macro behavior of property crime rates between 1975 and 1996 in terms of volume of crimes committed.[52]

Lochner (2004) considers a partial-equilibrium OLG model of crime and endogenous human capital, wherein an agent must choose how to allocate his time each period between work, crime, and human capital accumulation. Agents differ in human capital acquisition ability and criminal ability, with the expected outcome that young agents with low skill levels are the most likely to commit low-skill street crimes, and those with high skill profiles are more likely to commit white collar crimes (if any crime at all). Other models of crime and human capital include Huang, Laing, and Wang (2004)

[52]As an extension, İmrohoroğlu et al. (2004) attempt to capture the felon stigma effect using a permanent loss of a measure of income from legitimate activities after incarceration, raising the opportunity cost of crime and improving the model's fit to the prison education distribution. Unfortunately, their extended model does not reproduce the dynamics of the crime rate as well as the base model.

which considers costly human capital accumulation in a search environment, where individuals choose between working in the criminal and legitimate sectors.

I extend the OLG environment of İmrohoroğlu et al. (2004, 2006) in a partial equilibrium setting to include two racial groups facing differing opportunities, a skill-stratified stratified labor market, and an endogenous, costly, one-time education choice facilitated by inter-generational inheritance. Bequest motives are excluded reduction to the dynasty case removes the ability to analyze generational effects.[53] I calibrate the model to two distinct steady states, for 1976 and 2008, and perform a counter-factual analysis by substituting the 1976 apprehension rate and mean prison sentence length parameters into the 2008 model.

Endogenizing the human capital choice in an intergeneration environment is critical to assessing the inequality impact of criminal policy, and the primary contribution of this paper. The strong correlations between crime, education, and race (Pettit and Western 2004) call into question the independence of these factors. To the extent that criminal policy affects imprisonment, it also affects agent's earnings and the benefits accruing to the agent's successors (which in this environment reduce to education opportunities). If the criminal policy impacts the education distribution of the next generation, incarceration policy may exacerbate or produce income inequality relative to the counter-factual result. In order to evaluate the effect of criminal policy on income inequality, one must account for its impact on the endogenous education choice made by the next generation. I demonstrate that a calibrated model with intergenerational

[53]Also, imprisonment is more meaningful in a model where lifetimes are finite.

inheritance and endogenous education choice can produce a realistic educational profile, as a platform for future work.

Another important departure from İmrohoroğlu et al. (2004, 2006) is my addition of race as a state. When evaluating inequality outcomes in the United States, the relationship between race and imprisonment should not be ignored. The two race states are calibrated to reflect the income opportunities available to whites and blacks at each level of education. I am not aware of any macroeconomic model of crime which treats race in the manner presented here. Combined with the endogenous education mechanism, the inclusion of race as a state represents a step towards a dynamic model of race-correlated income inequality and intergenerational poverty, engendered by a cycle of low educational attainment and incarceration.

The model in this paper also differs from that of İmrohoroğlu et al. (2004, 2006) by stratifying the labor market into three distinct levels. Agents in İmrohoroğlu et al. (2004, 2006) are either employed or unemployed, with felons earning proportionally less than their non-criminal counterparts forever after release from prison. This compensation structure does not allow for full evaluation of the inequality effects of incarceration, as the resulting income distribution among agents is unrealistically flat. Furthermore, the felon income penalty "bakes in" inequality between felons and non-felons, and is thus unsatisfactory for assessing inequality-based outcomes. To allow for greater flexibility in representing returns to education, I include both professional and working-class employment paths, as well as an unemployment state. As non-professional workers are disproportionally represented among criminals, labor market specificity is crucial to

proper measurement of the income effects of incarceration. Although data on felon employment and earnings are limited, evidence does show that with time and no additional convictions, felons overcome their income stigma and catch up to their pre-incarceration incomes. Finally, my crime and labor market structure differs from Lochner (2004), in that agents are not forced to choose between crime and legitimate work in each period. The majority of criminals are employed at the time of their offense.

Finally, aggressive legislation related to the possession, use and distribution of illicit drugs has dominated the criminal policy environment in recent decades. A large proportion of the incarcerated population has a drug-related conviction. Two difficulties impede the modeling of drug crime directly. First, the level of drug crime is not fully quantified in the data: unlike other types of crime which are reported by victims (such as property crimes), drug crime is frequently discovered in the course of another investigation. Therefore, no direct measure for the level of drug crime exists. Second, because drug crimes are often committed, discovered, and prosecuted in conjunction with other crimes, drug-related incarcerations are not easily separable from incarcerations for other crimes. The correlation between drug and property crime is quite high: for example, the Arrestee Drug Abuse and Monitoring Program found that in the 2010 sample selected, 89% of those arrested for property crimes tested positive for illicit drug use (ADAM II Report 2010). Because a large proportion of property crime is related to illicit drug procurement, use and possession, a model of property crime will indirectly capture a measure of the effects of drug-related criminal policy change.

3.2 Model

I utilize a partial equilibrium OLG model of criminal activity wherein agents chose each period whether to commit a felony and face a positive probability of being apprehended. Agents are ex-ante heterogeneous: those "born" into the model inherit a permanent race state, and resources based on the income of their "parent" in the periods preceding their model birth. As in İmrohoroğlu et al. (2004, 2006), crime amounts to larceny. Apprehended criminals are considered felons, and sent to prison immediately during the period the crime is committed; criminals not apprehended the same period are assumed to have "gotten away with it."

3.4.1 Preferences

Agents maximize

$$\mathrm{E}\left[\sum_{j=1}^{J} \beta^{j-1} \log(c_j^{r,i})\right] \tag{3.1}$$

where $c_j^{r,i}$ is the consumption of an agent of race *r*, age *j*, and education level *i*, and β is the subjective discount factor. Agents live *J* periods as adults; the share of race *r*, age *j* individuals in the population is given by μ_j^r, where $\sum_{j,r} \mu_j^r = J$. Likewise, the share of race *r*, age *j* individuals with education level *i* is given by $\gamma_j^{r,i}$, where $\sum_{i,r} \gamma_j^{r,i} = 1$.

3.4.2 Opportunities

Agents choose their level of education[54] $i \in \{1,2,3\}$ in period zero of their lives. Education levels 2 (high school) and 3 (college) cost *q(r,i)*>0. While a high school education in the United States is technically free, the implicit cost of lost wages and perceived barriers to success after high school may be significant for individuals with low levels of parental investment (inheritance). That so many American youth drop out of high school in a non-random pattern suggests a systematic mechanism. The state of an age *j*=0 agent is summarized by $S_{j=0} = (r, a_0)$, where $r \in \{1,2\}$ represents race and $a_0 \in A, a_0 > 0$ represents the new agent's level of parental investment (inheritance). a_0 measures the level of household resources available to the child to enable his education. Let $a_0 = \nu y^*$, where $\nu \in (0,1)$ is the measure of parent income mandatorily given to the child, and y^* is the discounted sum of the parent's labor income up to the time the new agent enters the model at parental age j^*. Therefore, education level *i* is attainable only if $a_0^r \geq q(r,i)$, and

$$a_1^r = a_0^r - q(r,i) \tag{3.2}$$

The cost of education $q(r,i)$ is allowed to differ between races; opportunity costs and the college financial aid environment may be unequal. There is no consumption or labor opportunity at age *j*=0.

The state of an age *j*=1,…,*J* agent with education *i* is summarized by $S_j^i = (r, a_j, e_j) \in \{1,2\} \times A \times E$, where $a_j \in A, a_j \geq 0$ represents savings, and $e_j \in E =$

[54]This is a major departure from İmrohoroğlu et al. (2004, 2006), where human capital is exogenously assigned.

$\{u, h, m\}$ represents employment state. We can think of an agent with $e=u$ as unemployed, with $e=h$ as in an hourly or working-class job, and with $e=m$ as in a managerial or professional-class job. This stratification of employment opportunities represents a major departure from İmrohoroğlu et al. (2004, 2006). Each period, an age j agent who is not currently in prison chooses his savings $a_{j+1}^{r,i} \geq 0$, his consumption $c_j^{r,i} \geq 0$, and whether to commit a crime $l^{r,i} \in \{0,1\}$. Individuals in prison cannot access their assets to finance consumption or change their asset position. The assets of an apprehended criminal are "frozen" during the period in which he goes to prison, accrue interest at rate r, and are available in the period after he exits prison. Individuals can both work and commit a crime during the same period, and can commit at most one crime per period.

Labor income and unemployment insurance payments for an individual of race r, age j, education level i, and employment state e is given by $y_j^{r,i}(e)$. Employed agents are assumed to work their full endowment of available hours. The initial employment state depends solely on education choice according to distribution $\Lambda_{j=1}^{r,i}$. Subsequent opportunities follow a Markov process described by transition probability matrices $\Lambda_j^{r,i} = \left[\lambda_j^{r,i}(e'|e)\right]$, where for example $\lambda_j^{r,i}(h|u) = \text{PR}(e_{j+1} = h|e_j = u)$. Unlike İmrohoroğlu et al. (2004, 2006), I allow the employment transition path to vary with race and education.

Let $\pi_v(r, j)$ represent the measure of race r, age j individuals committing a crime in a given period, and π_a represent the apprehension rate, or probability that someone

committing a crime is arrested, convicted and sent to prison. π_a is exogenous.[55] Criminals steal fraction α of the average agent's pre-tax current-period labor income.[56]

Assuming an agent has already made his period-zero educational choice (subject to his inheritance constraint), the budget constraint for race *r*, age *j* agents not committing a crime is

$$a_{j+1}^{r,i} = (1+R)a_j^{r,i} + (1-\zeta)y_j^{r,i}(e,\, f) - c_j^{r,i}(e) \tag{3.3}$$

and the constraint for those committing a crime is

$$a_{j+1}^{r,i} = \begin{cases} (1+R)a_j^{r,i} + (1-\zeta)y_j^{r,i}(e,\, f) + \alpha\bar{y} - c_j^{r,i}(e) & \text{with probability } (1-\pi_a) \\ (1+R)a_j^{r,i} \text{ and } \hat{c} = c_p + (1-\phi)(1-\zeta)\, y_j^{r,i}(e=u,\, f=1) & \text{with probability } \pi_a \end{cases} \tag{3.4}$$

where R is the rate of return on asset holdings $a_j^{r,i}$, $\zeta = \tau + \nu$ for agents of age $j \leq j^*$ and $\zeta = \tau$ for agents of age $j > j^*$, τ is chosen by the Government to balance its budget, $\bar{y}$ is the mean current-period income of individuals not currently incarcerated, c_p is consumption while in prison, and ϕ is the fraction of the period spent in prison according to the criminal policy in place. The same-period income from legitimate activities for a newly released felon equals his unemployed wage, for the fraction of the period after release. Individuals imprisoned for committing a crime surrender the employment income they would have received during their prison sentence to the government, and cannot be victimized themselves. I assume that no innocent individuals are apprehended.

Agents cannot borrow, and are thus liquidity constrained:

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[55] For a model where police expenditure (and thereby apprehension probability) is endogenous, see İmrohoroğlu et al. (2000).

[56] This involuntary insurance simplifies computation, and is tantamount to assuming criminals cannot target specific individuals. Unlike İmrohoroğlu et al. (2004, 2006), I spread the loss from theft among agents not currently in prison via the tax system.

$$a_j^{r,i} \geq 0,\ r \in \{1,2\},\ j = 1,\ldots,J,\ i = 1,\ldots,I \tag{3.5}$$

As agents lack a bequest motive, there will be no savings in the last period of life.

3.4.3 Government

A Government exists to administer the theft and unemployment insurance systems, and provide consumption for prisoners. Beyond in-prison consumption c_p, I abstract away from the costs of the criminal justice system. Government revenues are thus

$$\sum_{r,j,i,e} \tau\ \gamma_j^{r,i}\, y_j^{r,i}(e,\cdot)\ +\ \sum_{r,j,i,e,a} \left[\bar{y} + (1-\zeta)\,\gamma_j^{r,i}\phi y_j^{r,i}(e,\ f=1)\right] l_j^{r,i}(e,a) \tag{3.6}$$

where the first term is income tax revenue from all citizens, and the second term is pre-arrest income confiscated from apprehended criminals. Government expenditures are

$$\sum_{r,j} \pi_v\,(r,\ j)\,\bar{y}\ +\ \sum_{r,j,i} \gamma_j^{r,i}\ y_j^{r,i}(e=u,\cdot)\ +\ \sum_{r,j} \pi_v\,(r,\ j)\,\pi_a\,c_p \tag{3.7}$$

where the first term is theft insurance compensation to individuals who are victimized, the second is unemployment insurance payments, and the third is consumption for those in prison. For simplicity, I assume the government knows who is victimized with certainty.

3.4.4 Equilibrium

I assume the existence of a stationary recursive equilibrium. Individuals may choose asset holdings corresponding to the points of grid *A*. For a race *r*, age *j*=1,…,*J*

individual with education i who enters the period in state $(a,\ e,\ f) \in A \times E \times \{0,\ 1\}$, the constraint set $\Omega_j^{r,i}(a,\ e,\ f)$ is the set of all three-tuples $(c_j^{r,i},\ a_j^{r,i},\ l_j^{r,i})$ such that Equations (3.2), (3.3), (3.4), and (3.5) hold, $c_j^{r,i} \geq 0$ for all $r \in \{1,2\}$, $j = 1, \ldots, J$ and $i \in \{1,2,3\}$, and a_0 is given. The consumer's problem can be represented as a finite-state, finite-horizon discounted dynamic program, with value function

$$V^{r,i}(a,\ e,\ 0) = \max\left\{V_{nc}^{r,i}(a,\ e,\ 0),\ V_c^{r,i}(a,\ e,\ 0)\right\} \tag{3.8}$$

where the value of not committing a crime in the current period is

$$V_{nc}^{r,i}(a,\ e,\ 0) \equiv \max_{c,a'} \log c^{r,i}(e) + \beta \sum_{e' \in E} \lambda^{r,i}\,(e' \mid e,\ 0)\, V^{r,i}(a',\ e',\ 0) \tag{3.9}$$

subject to constraints in Equations (3.2), (3.3) and (3.5), and the value of committing a crime in the current period is

$$\begin{aligned} V_c^{r,i}(a,\ e,\ 0) \equiv \max_{c,a'} (1-\pi_a) &\left[\log c^{r,i}(e) + \beta \sum_{e' \in E} \lambda^{r,i}\,(e' \mid e,\ 0)\, V^{r,i}(a',\ e',\ 0)\right] \\ &+ \pi_a \left[\log \hat{c} + \beta \sum_{e' \in E} \lambda^{r,i}\,(e' \mid e,\ 1)\, V^{r,i}((1+r)a,\ e',\ 1)\right] \end{aligned} \tag{3.10}$$

subject to constraints in Equations (3.2), (3.4) and (3.5).

A stationary recursive equilibrium for criminal policy $\{\pi_a, \phi\}$, income path $y_j^{r,i}(e,\ f)$, transition probabilities $\Lambda_j^{r,i}$, interest rate R, probability of felon stigma expiration η, and mandatory inheritance parameter ν, is a set of value functions $V_j^{r,i}(a,\ e,\ f)$, policy functions over education choice $I_j^{r,i}$: $A_0 \to \{1,2,3\}$, consumption $c_j^{r,i}$: $A \times E \times \{0,\ 1\} \to \mathbb{R}_+$, asset holdings $a_j^{r,i}$: $A \times E \times \{0,\ 1\} \to \mathbb{R}_+$, and crime activities $l_j^{r,i}$: $A \times E \times \{0,\ 1\} \to \{0,1\}$, age and education-dependent, time-invariant

measures of agents $\gamma_j^{r,i}(a,\ e)$ for each race $r \in \{1,2\}$, age j=1,…,J and $i \in \{1,2,3\}$, an aggregate crime rate and victimization probability $\{\chi, \pi_v\}$, and an average income from legitimate activities $\bar{y}$ such that

1. Given tax rate τ, victimization rate π_v, criminal policy $\{\pi_a, \phi\}$, interest rate R, probability of felon stigma expiration η, mandatory inheritance parameter ν, the educational budget constraint, age j budget constraints in Equations (3.3) and (3.4), and asset constraints in Equations (3.2) and (3.5), the individual policy functions $I_j^{r,i}$, $c_j^{r,i}$, $a_j^{r,i}$, and $l_j^{r,i}$ solve the consumer problem in Equation (3.1).
2. Average income from legitimate activities is given by
   $\bar{y} = \sum_{r,j,i,e,a,f} \gamma_j^{r,i}(a,\ e)\, \lambda^{r,i}(e,\ f)\, y_j^{r,i}(e,\ f)$.
3. The overall crime rate $\chi = \sum_{r,j,i,e,a} \gamma_j^{r,i}(a,\ e)\, l_j^{r,i}(e, a)$ is consistent with the probability of being victimized: $\chi = \pi_v$.
4. Given χ and $\bar{y}$, tax rate τ balances the Government's budget in Equations (3.6) and (3.7).
5. The time-invariant measure of agents $\gamma_j^{r,i}(a,\ e)$ for each race $r \in \{1,2\}$, age $j = 1, \ldots, J$ and $i \in \{1,2,3\}$ satisfy
   $\gamma_j^{r,i}(a',\ e') = \sum_{a,e} \gamma_{j-1}^{r,i}(a,\ e)\, \lambda^{r,i}(e' \mid e,\ f)$

3.3 Data and Calibration

Let J=24. We can think of agents at period j=0 as having real ages in 1-23, and at periods $j \geq 1$ as having real ages in 24-72. Each period $j \geq 1$ represents two physical years. New agents are "born" into the model at the end of parental period $j^* = 12$ and at physical parental age 48-49 (corresponding to physical parental age of 24-25 at infant birth).

Allow agents to commit crimes at ages $j \in (1,12)$ only, and assume that all crime occurs at the beginning of each period.[57] As the vast majority of all crime is committed by the young, this age restriction does not represent a significant departure from the data.[58]

Let $E = \{u, h, m\}$. Recall that agents choose their level of education at j=0, based on the lifetime discounted expected value of each level of education, and the education budget constraint. The system represented by Equations (3.6) to (3.10) is solved computationally by value function iteration over tax τ and the distribution of new agent inheritance.[59]

3.3.1 Calibration Data

In order to evaluate the impact of criminal policy change since the mid-1970s, I calibrate the model to estimate two distinct steady states using data for the pre-change

[57] This ensures agents can serve their entire prison sentence in the period a crime is committed.

[58] Of prisoners sentenced in 2009 under state or federal jurisdiction, 89.6% were aged 49 and younger (calculated using USDOJ Bureau of Justice Statistics, Prisoners in 2009, Appendix Table 13).

[59] Note that as the crime rate π_v affects government expenditure, iteration on τ is tantamount to iteration on π_v.

steady state (Calibration 1) and the post-change steady state (Calibration 2). As shown in Figure 3.1, incarceration rates prior to 1980 were relatively stable. Recent data suggest the prison population may be stabilizing at a new higher level, beginning in approximately 2008-2010 (see Figure 3.1). The goal of the counter-factual analysis is to capture the effect of the "tough on crime" legislation of the 1970s and 1980.[60] Therefore, Calibration 1 should reflect the state prior to the main impact of these policy changes. 1975 is the earliest year the CPS data are complete for personal income, education, and race. For Calibration 2, I will assume a new steady state was reached between 2008 and 2010. Both periods of interest span recessions characterized by macroeconomic uncertainty and international crises, albeit of very different natures; therefore, care should be taken in the calibration to avoid the undue influence of business cycle effects on the results. Among primary macroeconomic indicators, unemployment and interest rates seem most pertinent to the present model; Appendix Table C1 presents these statistics. Of the available years, 1976 and 2008 seem most similar in terms of unemployment and interest rates, and are therefore selected for analysis in order to minimize the impact of unrelated circumstances on the general result.

Data for 1977 and 2009 from the IPUMS-CPS database are used to calculate model parameters relating to income and opportunities, specifically the lifetime income path $y_j^{r,i}(e, f)$, initial employment distribution $\Lambda_{j=1}^{r,i}$, and employment state transition

[60] Major change to the American criminal justice system began in 1970 with the Comprehensive Drug Abuse Prevention and Control Act. It gained momentum in 1973 by the formation of the Drug Enforcement Administration, and in 1984 with the Comprehensive Crime Control Act. Many states passed mandatory 3-Strikes sentencing legislation in the 1990s.

matrices $\Lambda_j^{r,i}$, using data on education and job class. The major calibration target from the CPS data is the population measure of each education level by race and job class, as education is determined endogenously and impacts the distribution of employment types. The population consists of white and black men in the labor force aged 24 to 72; reported data include 1976 and 2008 income, unemployment, education, and occupation in both the income year and the survey year.[61] Men of other minority backgrounds are excluded for simplicity. In calculating the income path, individuals are classified as "white" if the variable "race" was coded as "white", and the individual was not also considered Hispanic.[62] Individuals are considered "black" if the variable "race" was coded as "black".

Occupation codes from the CPS variables "occ" and "occly", representing occupation at the time of survey and occupation during the year of income reporting (respectively), are used to classify workers as either white or blue collar in each year ($e \in \{h, m\}$).[63] CPS occupation categories changed slightly between the 1976 and 2008 data to reflect the changing workforce. Effort was made to ensure consistency between data years when classifying codes as either white or blue collar. This classification provides a good first estimate of the returns to college education experienced by the professional class. The third employment class (e=u) represents those unemployed or not

[61] Women are excluded, as men commit the vast majority of all crimes. See Figure 3.1.

[62] Individuals considered Hispanic include those indicating Mexican, Mexican American, Mexican American Indian, Chicano, Puerto Rican, Cuban, or Central American decent. Future work should take the Hispanic population into account.

[63] The assignment index is lengthy, and can be provided upon request. For example, in the 2008 data, occupation codes 80 (accountants and auditors), 305 (pharmacists) and 903 (aircraft pilots and flight engineers) were classified as white collar, while occupation codes 21 (farmers and ranchers), 724 (small engine mechanics) and 405 (food preparation and serving workers, including fast food) were classified as blue collar.

in the labor force (NILF), and is established using data for the income year and the survey year. An individual is classified as unemployed/NILF in the income year if he was unemployed for 39 weeks or more, or is listed as having no occupation type for that year (indicating NILF). Note that the data come from the March CPS survey. Therefore, in the survey year, an individual is classified as unemployed/NILF if he was unemployed on the survey date and had been for nearly all of the calendar year thus far (a duration of 10 weeks or more), or is listed as having no occupation type for the survey year.

Lifetime income path $y_j^{r,i}(e, f = 0)$ is constructed separately for whites and blacks using OLS regressions of constant-dollar individual earnings on indicators for high school and college diplomas, age in 2-year increments, age squared, type of employment during the income year, and a constant. Results are presented in Appendix Table C2, and all coefficients are extremely significant. Earnings projections are calculated using the OLS coefficients, inflated uniformly by year to ensure all elements of the income path are non-negative, and normalized by expressing all path elements as a fraction of the highest periodic 2008 income, on the unit interval. These show clearly increasing returns to college education between Calibrations 1 and 2. Graphical representations of lifetime income paths $y_j^{r,i}(e, f = 0)$ are presented in Appendix Figure C1.

Initial employment distribution $\Lambda_i^{j=1}$ for each steady state is calculated using the propensity of individuals aged 24-27 of each education level to obtain a job of class *e*. For example, of white high-school graduates aged 24-27 in 1976, 64% were working in a

job classified as blue collar. In order to ensure an adequate data pool and smooth outliers, transition matrices for adults already active in the model were estimated for two separate 24-year sections, for "young" agents (j=1...12) and "old" agents (j=13...24). Transition matrices $\Lambda^{r,i}_{j\in(1\ldots12)}$ and $\Lambda^{r,i}_{j\in(13\ldots24)}$ are produced using a pseudo-panel of job class in the income reporting year and the following year, based on CPS variables "occ" and "occly", for each education level separately. Felons are excluded from the professional class by setting the professional transition probability to zero, and proportionally redistributing the transition matrix between the unemployed/NILF and blue collar states. The initial distribution $\Lambda^{r,i}_{j=1}$ and transition matrices $\Lambda^{r,i}_{j}$ exhibit a high degree of persistence.[64]

Data from the Bureau of Justice Statistics and FBI Uniform Crime Reports are also used. The primary calibration targets for crime-related data include the overall property crime rate, and the educational attainment of prisoners (and thus criminals).[65] I set the apprehension rate π_a equal to the clearance rate (percentage of reported crimes "cleared" by the arrest of a suspect) times the conviction rate.[66] Clearance rates have remained stable from the mid-1970s to the present. However, the conviction rate, or ratio of convictions to charges brought, has increased markedly over the period in question: from approximately 75% in 1972 (Beale 1996) to 90% in 2008 (United States Courts

[64]Tables available upon request.

[65]Although one could argue that better-educated criminals may be more likely to avoid incarceration than poorly-educated prisoners, perhaps due to better legal defense, such factors are not likely to have a significant impact on the present model. Also, some of those in prison (in the real world) are innocent.

[66]İmrohoroğlu et al. (2004, 2006) use only the clearance rate as a proxy for the apprehension rate.

2008).[67] While arrest does not guarantee charges will be brought, the clearance rate times the conviction rate proxies the level of law enforcement activity and likelihood that a criminal will be incarcerated. The clearance rate for the 2008 estimation is 0.186, obtained from the FBI Uniform Crime Report for 2009, Table 25. The 1976 apprehension rate is 0.189, and is taken from İmrohoroğlu et al. (2004).[68] Conviction rates for 1972 and 2008 from the Federal court system are used in Calibrations 1 and 2, respectively. The fraction ϕ of the period which convicted criminals spend incarcerated equals mean time served for all property crimes: of a of a 2-year period, 12.5 months for 1976 (İmrohoroğlu et al. (2004) Table 6), and 21 months for 2008 (National Corrections Reporting Program[69]).

Consumption while in prison, c_p, is normalized to zero; I am not aware of any objective assessment of the relative value of consumption while deprived of one's liberty.[70] Because apprehended criminals do not go to prison for the entire period ($\phi < 1$), consumption in a period including a prison term ($\hat{c}$) is always positive. Data on felon employment are lamentably scarce, which complicates calibration. At present, there is no firm consensus among researchers on the magnitude and duration of the stigma suffered by felons. Western and Pettit (2010) estimate that felons experience a 40% reduction in earnings and high levels of unemployment after release from prison, which is especially

[67]Figures are for Federal courts. Conviction rates in state courts have followed the same upward trend. Legal experts believe this is related to increasing reluctance among prosecutors to bring cases in which they are unlikely to obtain a conviction (see Beale 1996). Ideally, my analysis would use the arrest-to-conviction ratio.

[68]See İmrohoroğlu et al. (2004) Table 6, figure for 1975.

[69]Mean time to first release from state prison for all property crimes, Statistical Table 3.3.10.

[70]İmrohoroğlu et al. (2004, 2006) treat this as a calibrated parameter, for different target dates. Apparently prison food leaves much to be desired.

significant in light of their finding that the average prisoner has a 10th grade education. Therefore, in the fraction of the period remaining after release from prison, felons are assumed to earn $\epsilon = 0.6$ of that earned by unemployed blue collar high school drop-outs.

Remaining determined parameters relate to the general environment and the inheritance mechanism. The rate of return on assets is set equal to the average 6-month secondary market CD rate as reported by the Federal Reserve Bank of New York for 1976 and 2008.[71] The common discount rate is set exogenously at 0.90, implying an intra-period annualized discount rate of 0.949. As the purpose of the endogenous education choice is to capture the trickle-down effects of incarceration on equality, the exact level of inheritance is only meaningful as it relates to education costs. This mechanism provides an indication of relative household resources available to children. Therefore, I normalize $\nu = 0.1$ and calibrate education costs in order to approximate the measure of individuals choosing each level of education by age 24 in the data.

Free parameters include crime "profitability" α, and high school $q(r, 2)$, and college $q(r, 3)$ education costs for whites and blacks. These are set by hand-calibration to best match the target moments listed, as well as produce a college-to-high school cost ratio that is not less than 1. The parameter space, not including initial distributions, transitions, and the income path, is presented in Table 3.1. Note that the calibrated costs for college education for blacks are less than costs for whites in both calibrations (more so for Calibration 1). This result is reasonable, and supported by the fact that student aid to blacks is both more common and more generous than that available to whites. For

[71] An average of dealer bid rates on nationally traded certificates of deposit.

Table 3.1: Calibration Parameters

Parameter	Role	Source/Target	Calibration 1: 1976	Calibration 2: 2008
Determined parameters				
ϕ	Fraction of period criminals go to prison	Mean prison time for property crimes	0.521	0.875
π_a	Chance of criminal capture and conviction	Clearance rate x conviction rate	14.2%	16.7%
ε	Consumption ratio after prison and under stigma	Findings of Western and Pettit (2010)	0.6	0.6
h	Likelihood a felon exits stigma state	Consensus of studies; see text.	0.3	0.3
β	Discount rate	None	0.9	0.9
r	Interest rate on assets	6-month CD rate per NY Fed	5.62%	3.14%
ν	Fraction of parent income given to child	Normalized	0.1	0.1
q(white,1)	Cost of dropping out of high school	Normalized	0.0	0.0
q(black,1)	Cost of dropping out of high school	Normalized	0.0	0.0
Free parameters				
α	Fraction of income stolen in theft	None; subject to calibration	0.221	0.221
q(white,2)	Cost of high school education, whites	None; subject to calibration	0.014	0.009
q(black,2)	Cost of high school education, blacks	None; subject to calibration	0.014	0.010
q(white,3)	Cost of college education, whites	None; subject to calibration	0.045	0.037
q(black,3)	Cost of college education, blacks	None; subject to calibration	0.034	0.035

Table 3.2: Model vs. Benchmark Data: Education and Employment by Race

	Calibration 1, Whites (1976)				Benchmark Data, Whites (1976)			
	Unemp/NILF	Blue Collar	White Collar	Totals	Unemp/NILF	Blue Collar	White Collar	Totals
HS Dropouts	0.064	0.179	0.025	0.268	0.072	0.175	0.027	0.274
HS Grads	0.086	0.356	0.170	0.612	0.066	0.343	0.215	0.624
College Grads	0.013	0.020	0.088	0.120	0.008	0.013	0.081	0.101
TOTALS	0.163	0.555	0.283	1.000	0.146	0.531	0.323	1.000
	Calibration 1, Blacks (1976)				Benchmark Data, Blacks (1976)			
	Unemp/NILF	Blue Collar	White Collar	Totals	Unemp/NILF	Blue Collar	White Collar	Totals
HS Dropouts	0.115	0.334	0.027	0.476	0.143	0.356	0.018	0.516
HS Grads	0.109	0.308	0.080	0.497	0.077	0.273	0.104	0.454
College Grads	0.005	0.005	0.017	0.027	0.003	0.004	0.023	0.029
TOTALS	0.229	0.647	0.124	1.000	0.223	0.633	0.144	1.000
	Calibration 2, Whites (2008)				Benchmark Data, Whites (2008)			
	Unemp/NILF	Blue Collar	White Collar	Totals	Unemp/NILF	Blue Collar	White Collar	Totals
HS Dropouts	0.029	0.023	0.003	0.054	0.029	0.043	0.002	0.074
HS Grads	0.111	0.389	0.058	0.558	0.099	0.403	0.079	0.580
College Grads	0.070	0.124	0.194	0.388	0.037	0.112	0.198	0.346
TOTALS	0.210	0.535	0.255	1.000	0.164	0.557	0.278	1.000
	Calibration 2, Blacks (2008)				Benchmark Data, Blacks (2008)			
	Unemp/NILF	Blue Collar	White Collar	Totals	Unemp/NILF	Blue Collar	White Collar	Totals
HS Dropouts	0.083	0.037	0.006	0.126	0.082	0.079	0.003	0.164
HS Grads	0.179	0.411	0.062	0.652	0.170	0.405	0.073	0.647
College Grads	0.057	0.059	0.106	0.222	0.026	0.073	0.091	0.189
TOTALS	0.319	0.507	0.174	1.000	0.277	0.556	0.166	1.000

example, in 2008, 91.9% of black students received student aid averaging $13,500, while 77.2% of white students received aid averaging $12,900 (Aud 2010). Data from earlier years reflect an even greater discrepancy in college assistance between races.[72]

3.3.2 Computation and Calibration Fit

The model converges for Calibrations 1, 2, and the counter-factual exercise to single steady states for many different (randomized) initial inheritance measures. Model results herein are based on computations done in MATLAB, using an asset grid on the unit interval with 500 grid points, dense at zero.[73] The upper-bound asset level on the grid is not binding.

Endogenous population distributions by race, age, education choice, and job class are compared with the data benchmarks in Table 3.2. For both calibrations, the model does remarkably well in approximating the total measure of individuals obtaining each level of education. For example, the maximum absolute difference in rate of educational attainment (summing across all employment types) between Calibration 1 and its benchmark is 4.2%, the difference between the high school graduation rate for blacks in the benchmark data (45.4%) and the endogenous model (49.7%). In Calibration 2, the maximum overall discrepancy in educational attainment is 4.2%, among white college graduates, with 34.6% graduating from college per the benchmark data, compared with

[72]For example, in 1986, blacks were 20% more likely than whites to receive aid, and their average award was 64% larger (calculated from Snyder and Hoffman 1990, Table 282). No doubt this reflects the greater need for financial assistance of the typical minority student.

[73]Data and code are available upon request.

38.8% in the endogenous model. All other differences in overall levels of education obtained are smaller than these.[74]

Among total employment types, absolute differences between the calibrated models and benchmarks are similar for Calibrations 1 and 2. The maximum deviation among Calibration 1 total employment measures is 4.0%, for white professional workers. For Calibration 2, the maximum deviation between the model and the benchmark is 4.9%; the model estimates 50.7% of blacks will be employed in blue collar occupations, while the benchmark data show 55.6% in that category.

Calibration vs. benchmark differences between education-by-employment cells followed patterns similar to those detailed above. In Calibration 1, the maximum inter-cell difference was 4.5%, for white professional workers with a high school diploma. In Calibration 2, the maximum inter-cell difference was 4.2%, for black blue collar workers without a high school diploma. Given the complexities of the multi-period model and the endogenous education choice, the model's fit to the data seems reasonable.

3.4 Results

The counter-factual exercise is conducted by substituting the Calibration 1 criminal policy (prison term probability π_a and sentence length ϕ) into the parameter space for Calibration 2, and computing the equilibrium. From Calibration 1 to Calibration 2, the prison term length increased 68%, from 52.1 % to 87.5% of a 2-year period, while the apprehension probability increased 18%, from a 14.18% to a 16.74% chance of

[74] This represents a significant improvement over an earlier J=2 model, which greatly overestimated the measure of individuals choosing college education, and underestimated those choosing a high school diploma.

Table 3.3: Crime Results for Calibrations 1, 2 and the Counter-Factual Exercise

	Calibration 1		Calibration 2		Counter-Factual
	Data	Model	Data	Model	Model
Property crime rate per two-year period	4.8%	5.2%	3.2%	3.1%	9.2%
Rate among whites		3.9%		2.7%	7.7%
Rate among blacks		18.9%		5.6%	19.2%
Incarceration rate	0.23%	0.38%	0.78%	0.45%	0.68%
Prisoner education and employment[1,2]					
Whites					
High school dropouts					
Unemployed/NILF	11.1%	35.4%	13.6%	25.8%	17.6%
Employed	38.8%	1.3%	43.9%	0.5%	0.3%
	49.9%	36.7%	57.5%	26.3%	17.9%
High school grads					
Unemployed/NILF	17.5%	54.7%	7.0%	69.8%	62.3%
Employed	29.8%	2.1%	30.3%	4.0%	2.6%
	47.3%	56.8%	37.3%	73.7%	64.9%
College grads					
Unemployed/NILF	0.0%	6.4%	0.9%	0.0%	16.7%
Employed	2.7%	0.1%	4.3%	0.0%	0.5%
	2.7%	6.5%	5.2%	0.0%	17.2%
Blacks					
High school dropouts					
Unemployed/NILF	17.3%	15.8%	21.4%	47.6%	28.8%
Employed	43.9%	55.7%	44.3%	0.7%	8.1%
	61.2%	71.5%	65.6%	48.3%	37.0%
High school grads					
Unemployed/NILF	12.0%	18.9%	7.1%	48.7%	42.1%
Employed	26.4%	9.2%	25.0%	3.0%	12.5%
	38.4%	28.0%	32.2%	51.7%	54.6%
College grads					
Unemployed/NILF	0.5%	0.5%	0.5%	0.0%	8.3%
Employed	0.0%	0.0%	1.8%	0.0%	0.1%
	0.5%	0.5%	2.2%	0.0%	8.5%

1 Calculated using data on 1974 state prison inmates serving time for property crimes, obtained from the Survey of Inmates of State Correctional Facilities, 1974. Data and code in Stata available upon request.

2 Calculated using data on 2004 state prison inmates serving time for property crimes, obtained from the Survey of Inmates in State and Federal Correctional Facilities, 2004. This survey is administered every 5 years; 2009 data are not yet available. Data and code in SAS and Stata available upon request.

incarceration after a crime. Table 3.3 presents the criminological results, and Table 3.4 the income and inequality results for both calibrations and the counter-factual.
In the steady state for Calibration 1, 5.2% of the total population (3.9% of whites and 18.9% of blacks) commits a property crime, compared with 4.8% in the benchmark data. In Calibration 2, 3.1% of the total population (2.7% of whites and 5.6% of blacks) commits a property crime, compared with 3.2% in the benchmark data. The model crime rate decreases between Calibrations 1 and 2, a feature consistent with the data; property crime rates are currently at a local low (see Appendix Figure C2).

Incarceration rates for Calibrations 1 and 2 (Table 3.3) are calculated by multiplying the model crime rate by the appropriate apprehension probability π_a and term length ϕ. Predicted crime rates imply that 0.38% (0.23% in data) and 0.45% (0.78% in data) of the population are behind bars at a given moment, for Calibrations 1 and 2 respectively. Note that due to data limitations, incarceration rate figures presented in the "data" columns of Table 3.3 relate to all offenses, not just property offenses. Direct comparison of model incarceration levels to the data is frustrated by the fact that because prisoners often serve time for multiple offenses, estimates of the incarceration rate for property crimes exclusive of other types of crime are not commonly reported by law enforcement agencies. Nonetheless, the property crime rate proxies the total level of incarceration in society; we should therefore expect the incarceration estimates herein to fall in the neighborhood of the data.

For Calibration 1, the predicted incarceration rate of 0.38% is slightly high as compared to 0.23% in the data. However, only 8% this increase is attributable to the

model's over-estimation of the overall crime rate. The difference between apprehension probability and the arrest-to-conviction ratio may upwardly bias the model incarceration rates produced by the model; apprehension probability includes the crime clearance rate (capturing arrests) and the post-charge conviction rate, but excludes the link between arrest and prosecution, and therefore assumes that all arrested individuals are also charged with a crime.

For Calibration 2, the predicted incarceration rate of 0.45% is quite low compared to 0.78% in the data. A likely source of this discrepancy is that, in the model, prison sentence lengths do not increase for repeat offenders. Three-strikes laws commonly require repeat offenders to serve the maximum statutory sentence without possibility of early parole (up to 25 years to life). Given the high rate of recidivism in the data and in both model calibrations, such repeat offender sentencing is bound to impact the overall incarceration rate following the criminal policy change. That the estimate for the Calibration 2 incarceration rate is too low, while the Calibration 1 estimate is too high, indicates this element of the policy change may have a meaningful impact on the results.

The education profile of the incarcerated population presented in Table 3.3 demonstrates that the model produces education-level heterogeneity in crime and incarceration. While the criminal education profiles do not match exactly to the data, the fact that the education profile changes appreciably between Calibrations 1, 2 and the counter-factual exercise confirms a relationship between the criminal policy and the endogenous education choice. Improving the employment type measures (perhaps by accounting for part-time employment) may result in a more accurate employment

distribution in society as a whole, and therefore better reflect the employment status of inmates prior to their arrest in Calibrations 1 and 2, and thus improve the reliability of the counter-factual results.

In Table 3.4, comparison of Gini coefficients shows that income inequality increases slightly between Calibrations 1 and 2, while inequality in asset holding increases significantly (from 0.655 to 0.894, a difference of 0.24). The increase in the Gini coefficient on assets reflects the increasing returns to college education over the period, and the related emergence of a wealthy, educated class. Average income $\bar{y}$ also increased by 43%. Rising average income makes crime more attractive to poorly educated agents; mean income has increased while the value of legitimate employment for high school drop-outs and graduates has not substantially increased. It is therefore somewhat remarkable that the crime rate falls over the period, both in the model and the data; this is reflective of the decreasing percentage of the population attaining the lowest level of education, and the more stringent criminal policy.

Table 3.4 also presents the ratio of income share to population share for various subgroups. For each subgroup, this ratio equals unity if the income share earned by the subgroup equals the population share of that subgroup; a ratio of less than 1 indicates that group accrues less than their population share of income. Comparison of Calibrations 1 and 2 reveals that income dispersion has become more equitable among blacks and the college-educated (ratios moving toward 1 in absolute value), and become less equitable among high school dropouts and graduates.

Table 3.4: Income and Inequality Results

	Calibration 1	Calibration 2	Counter-Factual
Gini coefficients:			
Non-institutionalized mid-life income	0.154	0.164	0.156
All mid-life income	0.168	0.174	0.180
Asset holdings	0.655	0.894	0.886
Ratio of income share to population share among*:			
Blacks	0.624	0.741	0.726
High school drop-outs	0.669	0.522	0.543
High school graduates	1.055	0.818	0.853
College graduates	1.546	1.366	1.427
Average income y-bar	0.394	0.562	0.536

*Ratio equals 1 if the income share earned by the subgroup equals the population share of that subgroup (equity).

Analysis of the counter-factual results shows that implementing the Calibration 1 criminal policy in the Calibration 2 parameter space has little impact on inequality, both in terms of the various Gini coefficients and the income shares for groups of interest. The impact of the criminal policy change on the justice system is quite significant: implementation of the Calibration 2 prison term length and apprehension probability result in a 200% increase in crime and a 51% increase in incarceration, as the costs of imprisonment for criminals decreased with the decrease in the prison term length. This suggests that the tightening of the criminal justice system has had a deterrence effect on crime. However, the counter-factual result may be significantly impacted by the criminal policy mechanism: as discussed above, a better estimate of the apprehension rate π_a may result lower crime and incarceration rates. Given the proclivity of prosecutors to bring

fewer cases to trial in 2008 as compared with 1974 (see footnote 19), obtaining a different result for Calibration 2 and the counter-factual under a different estimate of the apprehension rate would not be surprising.

3.5 Conclusion

The significant race and education disparities between America's prison population and free society suggest the relationship between incarceration, race and education is important to intergenerational poverty and criminal policy evaluation. I develop a partial-equilibrium OLG model of criminal activity with race, inheritance, and endogenous education to assess the inequality impact of the "tough on crime" legislation of the period from the 1970s to the 1990s. Although the model is able to produce a significant increase in wealth inequality between the 1976 and 2008 calibrations, the counter-factual exercise provides evidence that this inequality does not arise from the more stringent criminal policy environment currently in force. Instead, the observed 18% increase in criminal apprehension and a 68% increase in prison sentence length from 1976 to 2008 result in a marked decrease in crime and incarceration rates relative to the counterfactual exercise, and little impact on inequality under various measures.

When interpreting these results, readers should bear in mind the model's simplification of the criminal justice environment, including omission of increasing sentence lengths for repeat offenders, and the important link between arrest and prosecution. Too few apprehended criminals are employed under the present specification, which indicates that the incarceration effects of income inequality may be

understated. Modifications proposed for future iterations of this model should improve its fit to the criminal profile and improve the reliability of the results.

Open questions involve the impact of incarceration across generations. What kind of intergenerational differences arise from criminal policy changes like those of the late 20th century? The stationary nature of the current model limits its ability to assess intergenerational effects. Extensions examining the model transition paths between steady states, while interesting, are beyond the scope of this work.

Appendix A

Table A1: Estimated Effect of Unemployment Rates on Months to College Graduation, Allowing the Effect of the Post-College Job Market to Vary With Freshman Major

	Men		Women	
	Coefficient	Standard Error	Coefficient	Standard Error
Junior-year state unemployment rate for young workers with BA:	0.259	(0.629)	0.482	(0.388)
Freshman major:				
Undeclared	1.167	(1.494)	0.923	(1.146)
Humanities	-1.836	(1.879)	1.314	(1.442)
Life and physical sciences	0.160	(1.975)	-0.749	(1.328)
Engineering, computing and math	3.115	(2.265)	2.004	(1.343)
Education	-1.456	(3.500)	-0.263	(1.285)
Business	-1.362	(1.653)	0.993	(1.484)
Health	0.685	(2.433)	2.267	(1.368)
Professional and vocational technologies	-2.590	(1.745)	0.683	(1.197)
Interaction of unemployment variable with indicator for freshman major of:				
Undeclared	0.379	(0.535)	0.107	(0.344)
Humanities	1.069	(0.710)	-0.181	(0.516)
Life and physical sciences	0.722	(0.746)	0.669	(0.434)
Engineering, computing and math	0.433	(0.912)	0.00184	(0.476)
Education	1.734	(1.326)	0.338	(0.497)
Business	0.841	(0.624)	-0.589	(0.510)
Health	0.904	(0.962)	-0.0563	(0.493)
Professional and vocational technologies	1.670***	(0.602)	-0.458	(0.501)
Marginal effect of an increase of 1 percentage point in post-college unemployment, for students with a freshman major of:				
Undeclared	0.638	(0.397)	0.589**	(0.278)
Humanities	1.328***	(0.486)	0.301	(0.370)
Social sciences	0.259	(0.629)	0.482	(0.388)
Life and physical sciences	0.982**	(0.460)	1.152***	(0.383)
Engineering, computing and math	0.692	(0.568)	0.484	(0.508)
Education	1.993*	(1.189)	0.820**	(0.374)
Business	1.100***	(0.416)	-0.106	(0.455)
Health	1.163	(0.814)	0.426	(0.370)
Professional and vocational technologies	1.929***	(0.440)	0.025	(0.466)

Notes: OLS coefficients with in standard errors parentheses. The dependent variable is elapsed months from college entrance to graduation. Men and women are regressed separately. Unemployment rates for years 2-3 in college, for both in-college and post-college employment markets, are included together in each regression. Regressions include controls for normalized high school GPA, normalized SAT score (or translated ACT score), freshman year tuition, job hours worked while enrolled during freshman year, freshman expected family contribution and expected family contribution squared, as well as indicators for race in 4 categories, freshman major in 9 categories, whether either of the student's parents obtained a Bachelor's degree, urbanity in 3 categories, college state, and survey sampling strata. Data are drawn from the Beginning Postsecondary Students Longitudinal Study of first-time college students beginning as freshmen in 1995 or 2003, and limited to those entering at age 17-19 and graduating by June of their 6th year after entry. Sample restricted to US citizens beginning their studies at 4-year colleges located in the United States and DC, entering college in the fall, with non-missing start date, degree date, home state, institution type, graduation status, SAT/ACT score, race, and initial major.
Significance indicated by --*: P<0.10, --**: P<0.05, --***: P<0.01.

Table A2: Estimated Effect of Unemployment Rates on Months to College Graduation, Allowing the Effect of the Post-College Job Market to Vary With High-Income Status

	Men		Women	
	Coefficient	Standard Error	Coefficient	Standard Error
Junior-year state unemployment rate for young workers with BA	1.148***	(0.332)	0.612**	(0.241)
Family AGI in 3rd or 4th quintile during freshman year	0.682	(0.673)	0.940*	(0.525)
Interaction of unemployment variable with household income percentile during student's freshman year	-0.569**	(0.250)	-0.392**	(0.196)
Marginal effect of an increase of 1 percentage point in post-college unemployment, for students with:				
Family AGI in 1st, 2nd or 3rd quintile during freshman year	1.148***	(0.332)	0.612**	(0.241)
Family AGI in 4th or 5th quintile during freshman year	0.58*	(0.347)	0.221	(0.24)

Notes: OLS coefficients with in standard errors parentheses. The dependent variable is elapsed months from college entrance to graduation. Men and women are regressed separately. Unemployment rates for years 2-3 in college, for both in-college and post-college employment markets, are included together in each regression. Regressions include controls for normalized high school GPA, normalized SAT score (or translated ACT score), freshman year tuition, job hours worked while enrolled during freshman year, freshman expected family contribution and expected family contribution squared, as well as indicators for race in 4 categories, freshman major in 9 categories, whether either of the student's parents obtained a Bachelor's degree, urbanity in 3 categories, college state, and survey sampling strata. Data are drawn from the Beginning Postsecondary Students Longitudinal Study of first-time college students beginning as freshmen in 1995 or 2003, and limited to those entering at age 17-19 and graduating by June of their 6th year after entry. Sample restricted to US citizens beginning their studies at 4-year colleges located in the United States and DC, entering college in the fall, with non-missing start date, degree date, home state, institution type, graduation status, SAT/ACT score, race, and initial major. Significance indicated by --*: P<0.10, --**: P<0.05, --***: P<0.01.

Table A3: Estimated Effect of Unemployment Rates on Months to College Graduation, Allowing the Effect of the Post-College Job Market to Vary With Freshman Student Aid

	Men		Women	
	Coefficient	Standard Error	Coefficient	Standard Error
Junior-year state unemployment rate for young workers with BA	0.691**	(0.345)	0.371	(0.260)
Freshman year total student aid (in thousands)	-0.0636*	(0.035)	-0.0179	(0.035)
Interaction of unemployment variable with freshman year total student aid	0.0263**	(0.011)	0.0106	(0.012)
Marginal effect of an increase of 1 percentage pnt in post-college unemployment, for students with total aid of:				
None	0.691**	(0.345)	0.371	(0.260)
$1,500 (10th percentile)	0.730**	(0.338)	0.387	(0.251)
$6,400 (25th percentile)	0.859***	(0.320)	0.439*	(0.228)
$8,750 (50th percentile)	0.921***	(0.315)	0.464**	(0.223)
$13,600 (75th percentile)	1.048***	(0.311)	0.516**	(0.224)
$26,100 (90th percentile)	1.376***	(0.343)	0.649**	(0.291)

Notes: OLS coefficients with in standard errors parentheses. The dependent variable is elapsed months from college entrance to graduation. Men and women are regressed separately. Unemployment rates for years 2-3 in college, for both in-college and post-college employment markets, are included together in each regression. Regressions include controls for normalized high school GPA, normalized SAT score (or translated ACT score), freshman year tuition, job hours worked while enrolled during freshman year, freshman expected family contribution and expected family contribution squared, as well as indicators for race in 4 categories, freshman major in 9 categories, whether either of the student's parents obtained a Bachelor's degree, urbanity in 3 categories, college state, and survey sampling strata. Data are drawn from the Beginning Postsecondary Students Longitudinal Study of first-time college students beginning as freshmen in 1995 or 2003, and limited to those entering at age 17-19 and graduating by June of their 6th year after entry. Sample restricted to US citizens beginning their studies at 4-year colleges located in the United States and DC, entering college in the fall, with non-missing start date, degree date, home state, institution type, graduation status, SAT/ACT score, race, and initial major. Significance indicated by --*: P<0.10, --**: P<0.05, --***: P<0.01.

Table A4: Estimated Effect of Unemployment Rates on Months to College Graduation, Allowing the Effect of the Post-College Job Market to Vary With Small School Status

	Men		Women	
	Coefficient	Std. Error	Coefficient	Std. Error
Junior-year state unemployment rate for young workers with BA	0.889***	(0.313)	0.314	(0.228)
Indicator for freshman year college with enrollment in 1st quintile	-1.013	(0.814)	-2.484***	(0.838)
Interaction of unemployment variable with indicator for freshman year college with enrollment in 1st quintile	0.268	(0.289)	0.819***	(0.256)
Marginal effect of an increase of 1 percentage point in post-college unemployment, for students with:				
Freshman year college with enrollment in 1st quintile	1.156***	(0.421)	1.133***	(0.273)
Freshman year college with enrollment in 2nd-5th quintile	0.889***	(0.313)	0.314	(0.228)

Notes: OLS coefficients with in standard errors parentheses. The dependent variable is elapsed months from college entrance to graduation. Men and women are regressed separately. Unemployment rates for years 2-3 in college, for both in-college and post-college employment markets, are included together in each regression. Regressions include controls for normalized high school GPA, normalized SAT score (or translated ACT score), freshman year tuition, job hours worked while enrolled during freshman year, freshman expected family contribution and expected family contribution squared, as well as indicators for race in 4 categories, freshman major in 9 categories, whether either of the student's parents obtained a Bachelor's degree, urbanity in 3 categories, college state, and survey sampling strata. Data are drawn from the Beginning Postsecondary Students Longitudinal Study of first-time college students beginning as freshmen in 1995 or 2003, and limited to those entering at age 17-19 and graduating by June of their 6th year after entry. Sample restricted to US citizens beginning their studies at 4-year colleges located in the United States and DC, entering college in the fall, with non-missing start date, degree date, home state, institution type, graduation status, SAT/ACT score, race, and initial major. Significance indicated by --*: P<0.10, --**: P<0.05, --***: P<0.01.

Appendix B

This Appendix presents discrete duration analysis of time to graduation to complement the primary OLS analysis in the main paper. One benefit of the duration modeling approach is that it can eliminate any risk, however small, that the results could be influenced by unemployment-related dropout or stopout. It also accounts for the existence of right-censoring at 6 years follow-up in the data.[75]

I estimate the hazard of graduation in a given term using the classic right-censored discrete duration model following Jenkins (1995), with hazard function

$$g_{ik} = \text{prob}\left[T_i = k | T_i \leq k, W_{ij}\right] \tag{B1}$$

and related discrete-time log-likelihood function

$$\log \mathcal{L} = \sum_{j=1}^{n} d_j \ln g_{ij}(k\,|W_{ij}) \; + \sum_{j=1}^{n} (1 - d_j) \ln(1 - g_{ij}(k\,|W_{ij})) \tag{B2}$$

where i indexes the individual, k indexes the spell, T_i indicates time of spell end (graduation) for student i, d_j is the spell completion indicator, and W_{ij} contains individual-level covariates as well as unemployment data for the in-college and post-college markets. The model described by Equations (B1) and (B2) is implemented by OLS on the binary-outcome regression equation

[75] As discussed in the main text, I find no evidence that fluctuations in the in-college unemployment market impact student's likelihood of dropout by 6 years from college start. However, an increase in post-college unemployment in the freshman and sophomore years decrease men's chance of drop-out by 2.7% and 1.7% (respectively) per one percentage point increase in unemployment rates. Overall, it is unlikely that this small dropout response will bias the OLS results, as it is unclear that these incremental persisters will graduate within the 6-year period observed.

$$Y_{ist} = \sum_{j=1}^{4} \alpha_2^j UR_{s,t+j-1}^{PC} + \sum_{j=1}^{4} \alpha_3^j UR_{s,t+j-1}^{IC} + D_{ik}(k) + X_i\gamma + W_t\varpi + \eta_s + \rho_t + \varepsilon \quad \text{(B3)}$$

where *i* indexes the individual, *k* indexes the spell, *t* indexes the student's year of entry into higher education, and *s* indexes the student's college state. Dependent variable Y_{ist} equals 1 in the term the student graduates, and is 0 otherwise. Unemployment rates UR^{PC} and UR^{IC} are as described in the main text. $D_{ik}(k)$ represents the structural form of the hazard function, and can take nonparametric, log time or cubic polynomial form. As in Equation (1.1), X_i contains student-level covariates observed at the freshman year, W_t contains indicators for the survey strata of each survey wave, and η_s and ρ_t represent fixed effects for the student's college state *s* and year of entry into higher education *t*. I divide college completion into six-month terms beginning in January and July of each year, to avoid spurious results stemming from graduation timing differences between quarter- and semester-based institutions.

Results for Equation (B3) are presented in Table B1 below, for the nonparametric duration specification; results are similar when using the log time and cubic polynomial functional forms. When comparing Table B1 to the OLS results in Table 1.3, readers should keep in mind that a positive coefficient in the duration model indicates that variable increases the hazard a student graduates in a given period (that it, sign interpretation is opposite the OLS model).

Panel A considers the entire study sample of entering freshmen: those who graduate within 6 years, those who dropout without return to college within 6 years, and those who are still enrolled at 6 years. Results indicate that for men, higher post-college

unemployment during their freshman and sophomore years encourages earlier graduation, while higher post-college junior-year unemployment causes delay. Higher sophomore year in-college unemployment is also associated with lower hazard of college graduation (a longer college stay). For men, the coefficients on sophomore and junior year post-college unemployment are generally consistent with Table 1.3. For women, results in Panel A for the post-college unemployment market have the same sign as those for men, but do not rise to the level of statistical significance. Higher freshman year in-college unemployment is associated with a higher hazard of college graduation among women. Positive coefficients on freshman year unemployment for men likely stem from the relationship between men's time to drop out and freshman unemployment rates among those exiting school without return or degree within 6 years of college start.[76]

Panel B considers students who either graduate or are still enrolled at within 6 years from starting school. This population may provide a more relevant comparison group to graduates, as students who are still actively enrolled at the time of final survey may be more likely to eventually complete the degree than those who have exited without return by 6 years from entry. Results in Panel B resemble those in Table 1.3 more closely. For men, higher junior-year post-college unemployment causes delay, and the in-college market has no effect in any year. For women, unemployment in the post-college market yields a delay effect in the sophomore year, while freshman year in-college unemployment is remains associated with a higher hazard of college graduation.

[76] In separate analysis, I evaluate whether unemployment conditions impact how long eventual dropouts remain in college before exiting. I find that a percentage point increase in freshman year unemployment in the in-college market causes men to drop out 2.6 months (se 1.267) earlier *ceteris paribus*, but has no significant impact on how long women dropouts persist before exiting.

Table B1: Discrete Duration Estimates of the Effect of Unemployment Rates on Time to College Graduation

Panel A				
	Men		Women	
Mean state unemp. during student's	Post-College Market	In-College Market	Post-College Market	In-College Market
Freshman year	0.081**	0.070	0.014	0.092**
	(0.034)	(0.055)	(0.041)	(0.038)
Sophomore year	0.112**	-0.106***	0.046	-0.020
	(0.048)	(0.034)	(0.039)	(0.036)
Junior year	-0.140**	0.039	-0.032	-0.041
	(0.055)	(0.035)	(0.040)	(0.026)
Senior year	-0.053	0.024	-0.033	0.012
	(0.045)	(0.028)	(0.041)	(0.023)
Panel B				
	Men		Women	
Mean state unemp. during student's	Post-College Market	In-College Market	Post-College Market	In-College Market
Freshman year	0.015	-0.024	0.076	0.081**
	(0.034)	(0.051)	(0.055)	(0.041)
Sophomore year	0.082	0.039	-0.099**	0.004
	(0.055)	(0.041)	(0.044)	(0.032)
Junior year	-0.138**	-0.048	0.035	-0.026
	(0.057)	(0.045)	(0.036)	(0.027)
Senior year	-0.034	0.042	0.046	0.029
	(0.045)	(0.044)	(0.029)	(0.025)

Notes: Discrete duration model with in standard errors parentheses. The dependent variable is a 0/1 indicator for graduation in a given spell. Men and women are regressed separately. Panel A includes all students in the study sample. Panel B excludes students exiting college without return (dropouts). Unemployment rates for all years in college and both in-college and post-college employment markets are included together in each regression. Regressions include controls for normalized high school GPA, normalized SAT score (or translated ACT score), freshman year tuition, job hours worked while enrolled during freshman year, freshman expected family contribution and expected family contribution squared, as well as indicators for race in 4 categories, freshman major in 9 categories, whether either of the student's parents obtained a Bachelor's degree, urbanity in 3 categories, college state, and survey sampling strata. Data are drawn from the Beginning Postsecondary Students Longitudinal Study of first-time college students beginning as freshmen in 1995 or 2003, and limited to those entering at age 17-19 and graduating by June of their 6th year after entry. Sample restricted to US citizens beginning their studies at 4-year colleges located in the United States and DC, entering college in the fall, with non-missing start date, degree date, home state, institution type, graduation status, SAT/ACT score, race, and initial major.
Significance indicated by --*: P<0.10, --**: P<0.05, --***: P<0.01.

Appendix C

Figure C1: Normalized Lifetime Income Path
1976

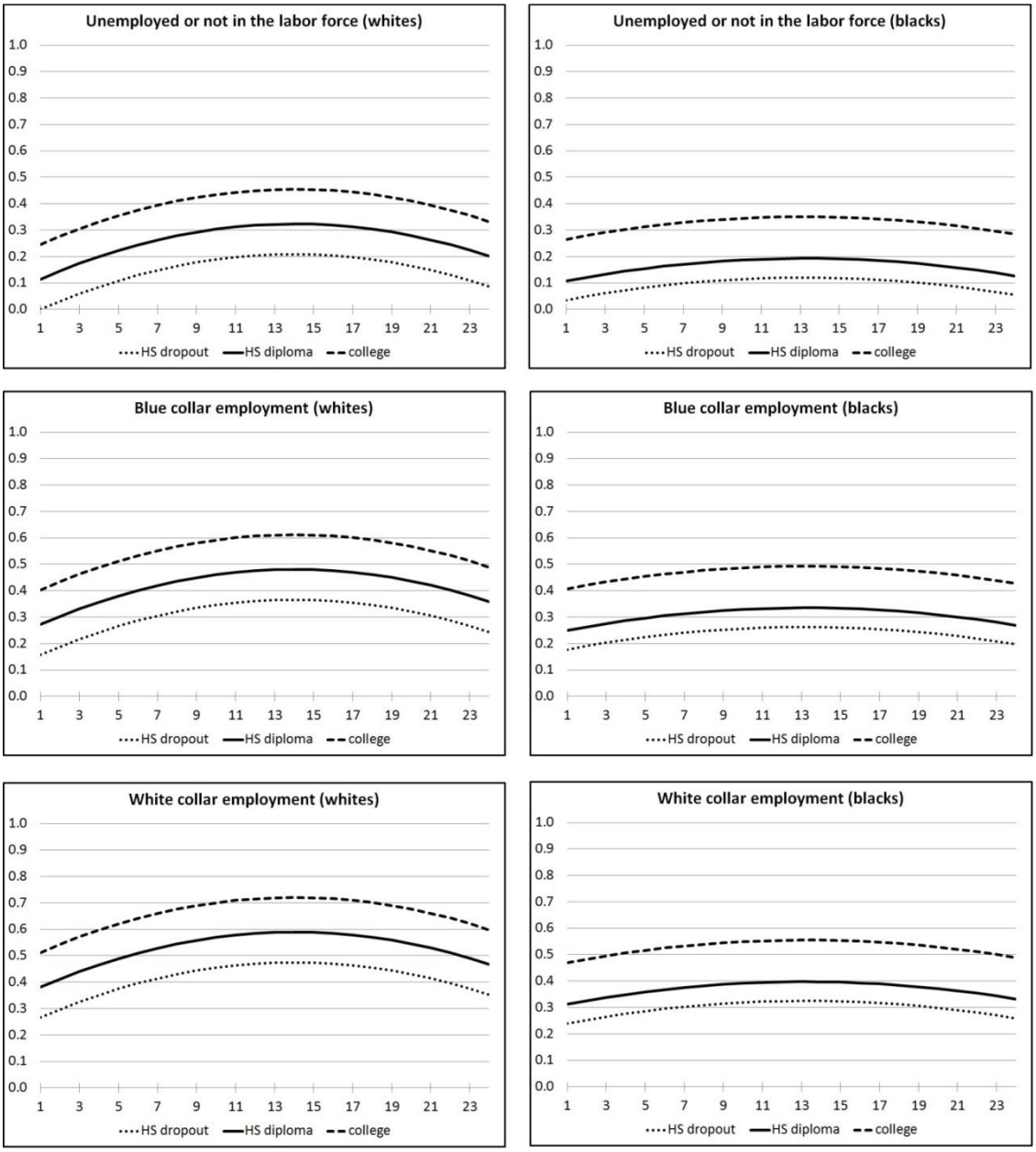

Figure C1 continues on the following page.

Figure C1, Continued: Normalized Lifetime Income Path
2008

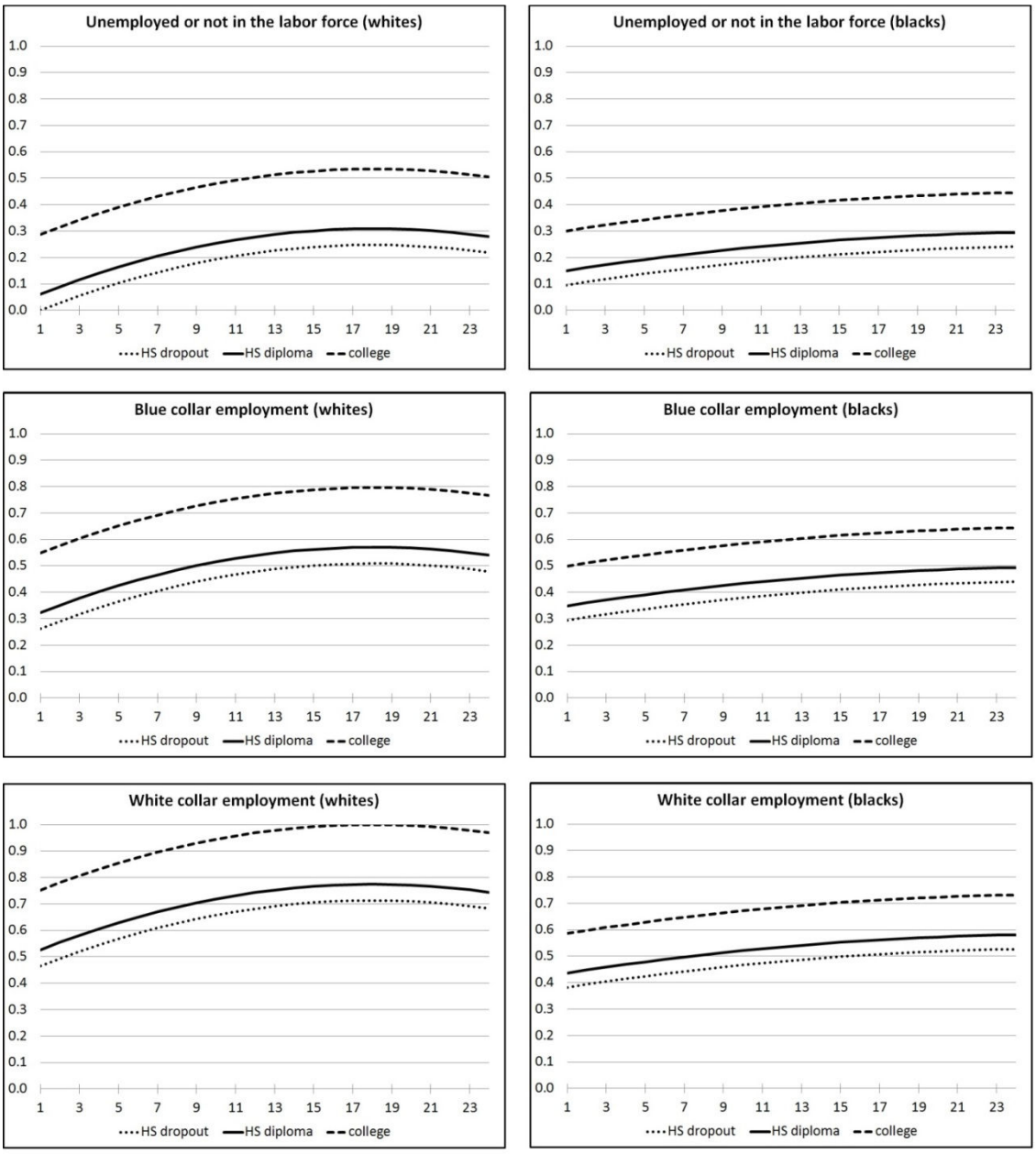

Figure C2: Crime Rates, 1975-2008

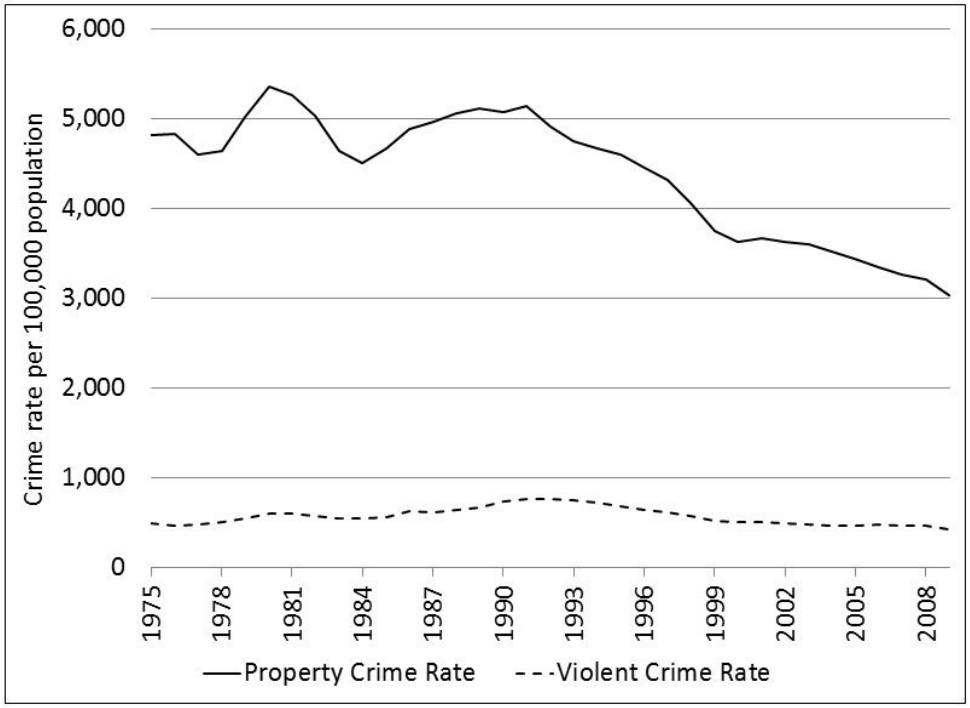

Table C1: Selected Features of the Calibration Periods

	Calibration 1		Calibration 2	
	Year	Rate	Year	Rate
Unemployment among those aged 25 and up:	1975	5.48	2008	4.83
	1976	4.83	2009	8.85
	1977	4.23	2010	8.95
6-month CD rates (secondary market):	1975	6.89	2008	3.14
	1976	5.63	2009	0.88
	1977	5.92	2010	0.44

Table C2: Personal Income Regression for Men Aged 24-72

	1976, Whites		1976, Blacks		2008, Whites		2008, Blacks	
	Coeff.	(se)	Coeff.	(se)	Coeff.	(se)	Coeff.	(se)
High school grads	12,172	(322)	7,699	(657)	6,503	(624)	5,754	(817)
College grads	26,124	(802)	24,438	(3020)	30,497	(864)	21,734	(1808)
Age	3,662	(85)	1,603	(166)	3,267	(139)	1,258	(203)
Age squared	-131	(4)	-61	(7)	-91	(6)	-24	(9)
Blue collar	16,689	(446)	15,102	(598)	27,762	(525)	21,106	(727)
White collar	28,241	(571)	21,716	(1146)	49,410	(888)	30,424	(1362)
Constant	-5,650	(538)	52*	(994)	-20,343	(949)	-8,234	(1166)
Observations	33,109		3,091		37,900		5,794	

Amounts in constant 1999 dollars.
Unless indicated, estimates are significant at the 1% level.

*Coefficient not significant.

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